$7.00 per annum
Single numbers $2.00

QUARTERLY

April, 1930
Volume III, Number 2

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

APRIL, 1930

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

THE EFFECT OF EMOTIONAL REACTIONS UPON RETENTION 197
By Luberta M. Harden

TESTS FOR COLOR-BLINDNESS: A SURVEY OF THE LITERATURE WITH BIBLIOGRAPHY TO 1928 222
By Istar A. Haupt

REVERSAL OF GALVANOTROPISM IN ECHINODERMS . . 268
By W. J. Crozier

TONE SHAPES: A NOVEL TYPE OF SYNAESTHESIA . . 277
By Michael J. Zigler

LOUDNESS AND TELEPHONE CURRENT 288
By L. F. Richardson and J. S. Ross

SHORT ARTICLES AND NOTES 307
Differential Adaptation of the Two Sides of the Retina (Dorothea E. Johannsen and Mason N. Crook) . . . 307
A Test of the Central Factor in Visual Adaptation (Mason N. Crook) 313
The Effect of Gravity on the Oscillations in the Path of an Ant (*Lasius flavus nearticus* Wheeler) (T. Cunliffe Barnes) . 318
On the History of Research with Rats and Mazes: A Collection of Notes (Walter R. Miles) 324
A Graphical Method of Fitting Curves to Learning Data (Willard L. Valentine) 337
The Comparative Legibility of Black and Colored Numbers on Colored and Black Backgrounds (Ruth Miyake, Jack W. Dunlap, and Edward E. Cureton) 340

BOOKS 344
E. G. Boring, "A History of Experimental Psychology"; C. Murchison [Ed.], "The Foundations of Experimental Psychology" (R. M. Ogden) 344

Worcester, Massachusetts
Copyright, 1930, by Clark University
Entered as second-class matter December 1, 1927, at the post-office at Worcester, Mass., under the Act of March 3, 1879

THE EFFECT OF EMOTIONAL REACTIONS UPON RETENTION*[1]

From the Psychological Laboratories of Clark University

LUBERTA M. HARDEN

INTRODUCTION AND STATEMENT OF THE PROBLEM

The question involved in the present paper may be considered as a phase of the general problem of retroactive inhibition. Specifically, our problem is to determine the effect of interpolated emotional reactions upon retention. Skaggs (7) has suggested that this problem be designated emotional retroaction, thus allowing a stricter definition of the general concept of retroactive inhibition. However, since the term retroaction seems rather misleading as a descriptive term, the writer prefers to use the term *emotional interference*. This term has no necessary implication of a backward reference, but may be used to refer to any effect which may be revealed in the recall records obtained under the experimental conditions in which retention is tested after an emotional disturbance.

Although no experimental work has been done which bears directly upon the present problem from a general psychological point of view, studies have been made by psychiatrists of the phenomenon of retrograde amnesia, i.e., a complete loss of retention caused by a physical shock or severe emotional disturbance. Among psychologists this phenomenon has received no more than casual mention. Skaggs (7) and Robinson (6) both mention retrograde amnesia as unusual instances of positively accelerated forgetting. On the other hand, Stratton (9) reports many examples of retroactive hypermnesia, i.e., instances of an unusually vivid memory of events occurring prior to some great shock.

Perhaps the few experiments in which Robinson (6) gave as interpolated activity the study of pictures of nudes correspond more closely

*Recommended by Walter S. Hunter and received for publication by Carl Murchison of the Editorial Board.

[1]The experimental work described in this paper was carried out under the direction of Professor Walter S. Hunter, and the writer hereby gratefully acknowledges his guidance and assistance.

to the present study than any others that have been reported. Robinson's purpose was to furnish an activity with some affective tone. Landis (4, 5), also, in his studies of emotional expression, used similar stimuli to arouse emotional reactions. Whether or not we describe the reactions to such stimuli as emotional, the interpolation of such reactions introduces a factor not found in ordinary reading or in the learning of numbers or syllables. The term emotional reaction will be used in this paper to refer to a general bodily disturbance. The stimuli used to arouse such reactions in the experimental situations were, of course, not so violent as those which produce the retrograde amnesia of the psychiatrists.

The general experimental plan used in all the problems of this study was that followed by investigators in the general field of retroactive inhibition: comparison was made between the results of two tests, one in which the rest or control period was interpolated between learning and recall, the other in which the emotional stimuli were so interpolated. The latter will be referred to in this paper as the test situation. The comparison of results obtained from the two tests may be made (1) in terms of percentage of loss where the recall is measured both before and after interpolation, or (2) in terms of recall under control and test conditions when the record of recall is taken only after interpolation. Both plans were used in the present study.

Preliminary Experiments

Two preliminary experiments were made in order to map out the field and to perfect the method of investigation. Inasmuch as the results secured were in harmony with those of the main experiments, only a brief description of them will be given. In the first experiment the problem was to determine the effect of the unexpected stimulus of strong odors upon the retention of naïve subjects. Twelve college men served as the subjects. The recall method was used and comparison was made between records taken before and after interpolation. Fifteen nonsense syllables were exposed five times (two seconds' exposure per syllable) and written recall was taken after each exposure and again after interpolation. The record of the last recall before interpolation was used as the basis for computing the loss during the interpolated interval; the other recalls before interpolation were considered as trials in the learning process.

The interpolated period was three minutes in length. During the

control interval the subject read aloud from an ordinary prose selection. During the test interval the subject was given a group of nine bottles containing odorous liquids, all of which were agreeable but two, ammonia and ammonium bisulphide, which were Numbers 7 and 9 in the series, and served as the disturbing stimuli in this experiment. The subject was instructed to sniff at each bottle and, if he knew what the odor was, to mention the name of it. The reaction to Odors 7 and 9 was much the same for all subjects and consisted in a sudden start and a quick sidewise movement of the head.

The results show an average percentage loss of 12.8 for the control experiment, and one of 13.7 for the test experiment (*M. V.*'s 15 and 13%, respectively). The individual records varied greatly, from 0 to 50%, in the percentage of loss shown in the two situations. Five subjects showed no loss on either control or test records, four showed more loss in the test record, and three showed more loss in the control record.

In the second experiment a more complex exciting situation was used in the interpolated interval of the test experiment. This situation involved the sudden loss of bodily support, the crash of falling weights, and a brilliant flash of light. Eleven men from the elementary psychology class served as subjects. The recall method was used as in the preceding experiment, and the general experimental plan was the same. The series of 15 nonsense syllables was exposed six times, but written recall occurred only after the sixth presentation and again after interpolation instead of after each exposure as in the first experiment. During the three-minute interpolated period of the control situation the subjects read aloud as before. In the interval interpolated in the test experiment, the subject was instructed to lean back in his chair, relax, and repeat the alphabet as rapidly as possible. While this was going on, the experimenter caused the back of the chair to fall by pulling some weights off a support on the back of the chair; the weights, in turn, pulled the rod supporting the chair-back out of its grooves and then fell to the floor with a crash. At the same time an Eastman flash cartridge was lighted. The reactions to this disturbing situation were much more striking than those in the former experiment.

The averages of the percentage loss in the control and test situations are very nearly the same, 15.9 for the control situation and 17.8 for the test situation (*M. V.*'s, 12% in each case). A study of the individual records shows that two subjects show no loss in either the

control or the test situation, one shows the same loss in both, three show more loss in the control situation, and five show more in the test situation.

From the results obtained in the preliminary experiment we have gained no positive evidence of an emotional effect upon retention. It is only in the light of our further experimentation in which similar results were obtained with greater numbers of subjects that the slightly greater losses reported in the test situations of these two experiments take on any significance.

A Note on the Reliability of the Method Used in the Preliminary Experiments. In order to evaluate the results of these two experiments with respect to the method by which they were obtained, the reliability of the method was computed. A reliability coefficient of $.84 \pm .025$ was obtained by the retest method with a group of 52 elementary history students. The interval between tests was 84 days. The same series of 15 nonsense syllables (exposed three times) was used in each test and conditions and instructions were the same. Since the coefficient is relatively very high, we may assume that the results possess a fair degree of reliability.

The Main Experiments

Experiment 1

Problem. The purpose of Experiment 1 was to determine the effects of five interpolated activities upon retention when the subjects were acquainted with the general nature of the experiment. The activities will be described specifically later, but may here be indicated by descriptive titles: (1) reading silently with lip movement (control activity), (2) reading aloud from a humorous selection, (3) reading with noise and flash, (4) reading with electrical shock, and (5) reading silently with electrical shock or threatened shock, loss of bodily support, and noise.

Subjects. Ten subjects served in this experiment. One was a senior in Clark College and had served in the preliminary experiments the previous year. He therefore knew the general nature of the problem. The other subjects were graduate students in the Department of Psychology. Six of these had heard the preliminary problems reported in class. The other three were informed that the purpose of the experiment was to study the effects of emotional reactions upon retention, but they were given no information as to the exact situations to which they would be subjected. Each subject had

preliminary practice periods weekly at his regularly appointed hour for two weeks before the experiment proper began. At these sittings each individual studied one list of each type of material, i.e., nonsense syllables, numbers, and words. The interpolated interval in each case was occupied with reading silently with lip movement. In the experiment proper each subject served five times and, after the first time, of course, he knew more or less what could be expected, although he never knew what interpolation was to be used at any given time.

Method. The subject was seated in a Morris chair on the opposite side of a small table from the experimenter and in front of the exposure apparatus (the Spindler-Hoyer memory drum). At the experimenter's left, between her and the subject, was a board screen which allowed the experimenter to manipulate records and apparatus without disturbing the subject. On the right side of the exposure apparatus was a table lamp so shaded as to light only a small area at the exposure window. At the subject's left was a floor lamp which the experimenter could manipulate. When the subject was reading, this lamp was turned on and furnished illumination at the proper angle for reading when the subject sat back comfortably in the chair. This light was turned off when the subject was studying the lists exposed on the memory drum.

Three types of learning material were used, nonsense syllables, three-place numbers, and three-letter words. Twelve syllables comprised a list, while 10 numbers and 10 words made up lists of those materials. The syllables were chosen from the lists of syllables which Glaze (2) found to have 50% or less association value. Words were taken from Cason's (1) list, while the numbers were constructed at random except that in no case did the digits appear in regular ascending or descending order, nor did arrangements such as 2, 4, 6, or 3, 6, 9, or vice versa, occur.

The recall method was employed and exposure was made visually. Nonsense syllables lists were shown six times, with an exposure time of two seconds for each syllable and an interval of 14 seconds between each two of the six presentations. For the number and word lists the exposure time for each member of the lists was the same as that for the syllables, but the interval between presentations was 18 seconds in length. The number series were shown five times, the word series, twice. Recall was requested only after interpolation.

When the subject came into the experimental room he was asked

to read the following instructions which always lay on the table beside the apparatus.

INSTRUCTIONS

For the best interests of this work I request that you mention this experiment to no one—and we shall not discuss it in here.

You will be shown a list of nonsense syllables, words, or three-place numbers. Study each item of the list and pronounce it *aloud* as it is exposed, for I shall ask for a written recall later. Words will be shown twice, numbers five times, and nonsense syllables six times.

After the last exposure of whichever list we are working with, I shall give you some reading material from which I wish you to read *with lip movement* until I ask you to stop.

Immediately after the stop signal on the reading you are to write as much as you can (regardless of order) of the list studied. Do not begin to write until I have given the "Ready, write" signal, and please say "Now" when you have finished, for I shall time you on this recall.

The subject was now told what material would be shown first and what the number of exposures would be. The material was exposed the prescribed number of times, the machine was stopped, and the subject was put through one of the five interpolated activities and then asked for the recall. This procedure was repeated with a list of each of the materials at each sitting. Time was recorded with a stop-watch from the moment the experimenter said, "Ready, write" until the subject said, "Now." In scoring the written recall records, one point was given for each syllable, word, or number correct in kind. One-half credit was given for two letters or digits correct.

As stated above, five activities were used as interpolations. The length of the interpolated interval was two minutes in all cases.

Activity 1. Reading. This situation corresponded to the usual rest period and was the control situation in this experiment. The material was always the same passage taken from Wood's *The Influence of Monarchs.*

Activity 2. Reading aloud from a humorous selection. For this activity the subject was handed a typewritten copy of "The Heedless Horseman" by Weare Holbroke or "The Garter" by Dorothy Parker and was asked to read aloud until the experimenter said to stop. No subject ever read the same selection from these articles more than once.

Activity 3. Reading with noise. Immediately after the

learning period, the subject was handed *The Influence of Monarchs* and began to read. A revolver was fired under the table just as soon as the experimenter could do so after handing the book to the subject. In some of the experiments a flash lamp was set off back of the experimenter and facing the subject. This was not used in all of these situations because of the excessive amount of smoke which it produced in the small experimental room. Toward the end of the two-minute period the revolver was fired again.

Activity 4. Reading with shock. In this case, before the subject was given *The Influence of Monarchs,* leather wristlets were placed on his wrists. In these wristlets copper strips were woven which were connected with the electric lighting system through a rheostat and a switch. The connecting wires were long enough to allow the subject perfect freedom of movement. By means of the rheostat and switch the experimenter could control the intensity and frequency of the shocks. The shocks were given at approximately 30-second intervals, beginning as soon as the subject started to read. A constant intensity of about 70 volts was used throughout this experiment.

Activity 5. Reading with noise, loss of bodily support, and shock or threatened shock. The subject was seated in the Morris chair, as in all the experiments. The seat of this chair was hinged at the front as well as at the back, and was held in position by a support which the experimenter could cause to collapse by pulling a small rope. This allowed the seat to drop 10 inches at the back. By the same pull of the string which collapsed the support under the chair, the rod supporting the back of the chair was pulled up, allowing the chair-back to fall simultaneously with the fall of the seat. The rod connecting the ends of the backward extensions of the chair-arms prevented the back from falling to the floor. The wristlets used in Activity 4 were attached in this situation also; the subject again read aloud from the same selection from *The Influence of Monarchs,* the current was turned on two or three times during the two-minute interval, and the chair collapsed about seven minutes before the end of the period.

As stated above, each subject learned three lists of material at each experimental session. After the study of each list he engaged in one of the five activities just described. Since five sessions were thus necessary in order to have a record of recall for each type of material after each interpolated situation, some precaution had to be

taken to equalize possible practice effects. To accomplish this a program was drawn up as shown in the following schedule.

Group	Materials	Days I	II	III	IV	V
	Syllables	1	4	2	5	3
A	Numbers	2	5	3	1	4
	Words	3	1	4	2	5
	Syllables	2	5	3	1	4
B	Numbers	3	1	4	2	5
	Words	4	2	5	3	1
	Syllables	3	1	4	2	5
C	Numbers	4	2	5	3	1
	Words	5	3	1	4	2
	Syllables	4	2	5	3	1
D	Numbers	5	3	1	4	2
	Words	1	4	2	5	3
	Syllables	5	3	1	4	2
E	Numbers	1	4	2	5	3
	Words	2	5	3	1	4

"Groups" here represent but two persons. "Days" refer to the experimental days, which were planned to be one week apart, but which were in two cases as far as three weeks apart, and in four cases two weeks apart. Arabic numbers refer to the interpolated activity used after the studying of the type of material indicated in the second column. This arrangement equalized practice factors in relation to materials and situations.

It was not found possible to follow this arrangement precisely. The most outstanding exception was the substitution of Activity 2 for Activity 5 in three cases in the early part of the work, which necessarily brought a number of the latter situations to the last sessions.

The order of presentation of the lists was varied in an irregular manner. Usually if a control activity (Activity 1) was called for on the day's schedule, the experiment in which it was to be used was given first. On the other hand, usually when Activity 5 was called for, the experiment utilizing that situation came last because of the necessity for rearranging the chair after that situation. This plan of using Activity 1 first and Activity 5 last was not always followed, for it seemed undesirable to allow any subject to get cues as to what would happen, and it seemed that he might learn to suspect the order of the day's series.

Results. We now turn to the examination of our experi-

mental results and shall first describe the reactions of the subjects to the interpolated situations. The reaction of all the subjects to Activity 1 was normal. After studying the material which was to be learned, they were told that the interpolated period was to be a "rest" period, i.e., that nothing would happen while they read. Activity 2 was not successful in all cases in bringing out the laugh response, although it was very successful with some subjects. If the subject showed any inclination to laugh, the experimenter laughed very freely. All subjects at least smiled. With Activity 3 all the subjects started at the revolver report. Extra loud blanks were used, and, since the room was small, the report was quite deafening. The smoke was also a disturbing factor and nearly all the subjects remarked about its disagreeableness. The electric shock used in Activity 4 was by far the most disturbing stimulus used in the entire series. The strength of the current was sufficient to draw the arms up with considerable force. The subjects often exclaimed. Practically all of them took the precaution to wipe their hands dry before the wristlets were put in place, but all perspired rather freely; the hands of Subjects 5 and 6 were always unusually wet when the wristlets were removed. Individual reactions will be described in connection with later results. In Activity 5 the reaction to the shock was the same as that just described. The reaction to the noise and fall was not particularly marked. Subject 2 reported a decided feeling of relief in one case when the chair collapsed because she then knew from former experiences that there would be no more shocks in that particular interval.

Comparisons may now be made of results obtained under control and test conditions on the basis of (1) the percentage of correct recall, (2) the total time taken for recall, and (3) the time taken per syllable correctly recalled (total time for each individual divided by his recall score). The recall data are presented in Table 1, where are shown the average percentages of correct recall under each of the five situations, i.e., following the interpolation of each of the five activities described above. It should be remembered that the test employing Activity 1 was the control experiment. The records for the four test situations have been averaged and presented in the last column.

An examination of these data reveals that in 8 out of the 12 recalls after test activities (Columns 2, 3, 4, and 5) there was a greater loss than in the corresponding control experiment (Column

1). Two of the four records which show greater recall in the test situation were made after the interpolation of Activity 5. As pointed out above, Activity 5 interpolations were somewhat concentrated at the end of the experimental period. It might therefore appear that the tendency to emotional effects upon recall decreased with practice. However, in Experiment 3 data will be presented which indicate the opposite effect.

The final column of Table 3 shows that the average of all recalls after the interpolation of Activities 2-5 is less than that of the corresponding control record shown in Column 1. However, our group was extremely small and no statistical significance can be ascribed to these differences. The most that we can say is that there is a consistent tendency to slightly poorer recall after interpolated disturbing conditions than after interpolated reading.

Turning now to the time records presented in Tables 2 and 3, we find tendencies toward greater total recall and a longer recall time for syllables and words under the test conditions. So far as consistency of results is concerned, we may point out that 10 of the 12 test records (Columns 2, 3, 4, and 5) in Table 2 are greater than their corresponding control records (Column 1). In Table 3 there is no test record which is less than its corresponding control record,

TABLE 1

AVERAGE PERCENTAGE OF CORRECT RECALL (N = 10)

Materials	Activity 1	2	3	4	5	2-5
Nonsense syllables	57	46	54	48	58	51
Numbers	53	49	48	47	60	52
Words	74	66	76	77	66	71

TABLE 2

AVERAGE TOTAL RECALL TIME IN SECONDS (N = 10)
($M.V.$'s in parentheses)

Materials	Activity 1	2	3	4	5	2-5
Nonsense syllables	49	57	49	70	65	60
	(19)	(25)	(23)	(47)	(44)	(35)
Numbers	41	42	36	35	47	40
	(19)	(19)	(14)	(11)	(16)	(15)
Words	34	46	39	42	44	43
	(14)	(18)	(15)	(13)	(20)	(17)

TABLE 3

AVERAGE RECALL TIME PER ITEM CORRECTLY REPRODUCED ($N = 10$)
(*M.V.*'s in parentheses)

Materials	Activity 1	2	3	4	5	2-5
Nonsense syllables	7	10	8	14	9	10
	(3)	(3)	(5)	(9)	(3)	(4)
Numbers	8	9	9	8	9	9
	(2)	(4)	(5)	(3)	(3)	(3)
Words	5	7	5	6	7	6
	(2)	(3)	(2)	(4)	(4)	(3)

although two are equal to that record. As in the case of the recall scores, however, we place no importance on absolute figures because of the small number of subjects and the large *M.V.*'s; we wish to call attention only to general trends.

It will be interesting to examine a few individual records in cases where there were particularly striking reactions to the disturbing stimuli. Subject 9 had a great antipathy for the shock situation during the early part of the work. When the wristlets were put on for the third time he was much disturbed and said that he didn't believe he could remember a number and that it wasn't necessary to turn on any current. As a matter of fact, at the end of the two-minute interval he did not recall any number correctly. Subject 6 gave a similar suggestion when the wristlets were put on, as if to avoid the situation. He recalled 2.5 numbers in 15 seconds as compared with 4 in 12 seconds under the control situation. Subject 5 seemed never to be particularly disturbed by anything, but in one case the experimenter accidentally moved the switch down in pulling the string to collapse the chair and the current was left on. The subject appeared to be greatly disturbed. He recalled two syllables in 25 seconds as compared with five in 38 seconds in the control situation.

Conclusions. Because of the small number of subjects used in this experiment, the results must be regarded as suggestive rather than conclusive. However, we may conclude tentatively that there is evidence for a slight interference from the disturbing situations. This is shown by the rather consistently poorer recall record, the longer total recall time, and the longer recall time per item recalled than were found under the control condition.

Experiment 2

Problem. The results of Experiment 1 were more suggestive than conclusive, and it seemed wise to continue the study with the use of a more sensitive method than that of recall and with one particular emotional situation rather than with several. Hence Experiment 2 was designed to determine more accurately by means of the right associates method the effect of Situation 5 of Experiment 1 upon a new and larger group of subjects. In the course of the experiment with the first two subjects it became evident that a more analytic timing method was desirable and the one described below was introduced.

Subjects. Twenty-nine elementary psychology students, all men, served as subjects. None of them knew the problem and only one had been in the experimental room prior to the time he came for experimentation. Only one had had any experience with nonsense syllables; he had been in the history group which took the two tests mentioned in the statement on the reliability of the recall test as used in the preliminary experiments.

Each subject was given a preliminary practice period in which he learned two lists of syllables. Appointments for half-hour sessions were made in class with the understanding that each subject should come at the day and hour stated for a period of two weeks.

Method. The right associates method was employed. Six pairs of nonsense syllables were used for each test series. These were presented serially on the Spindler-Hoyer memory drum. Each item of a pair was exposed 1.5 seconds, with no interval between. There was one blank exposure between pairs, i.e., an interval of 1.5 seconds. Five presentations were given with an interval of 10 seconds between presentations. Further details of the method are apparent in the instructions given to the subjects as follows.

INSTRUCTIONS

In general: This work will furnish a part of the data for my doctor's thesis and I wish to make two very earnest requests concerning it: (1) Please do not discuss what happens here with anyone—when your classmates ask you what it's all about, etc., just say it's a problem in learning and that I asked you to say nothing more about it. (2) Please do your best to learn the syllables which I shall show you. I am not in the least interested in personal records and am *certainly not* interested in, or trying to test, your intelligence. I shall, in fact, treat your records by number and not by name.

A series of nonsense syllables will be exposed on the exposure apparatus

in front of you. Please pronounce each syllable to yourself as it appears. You will note that the syllables are shown consecutively, but in this sort of rhythm, " " ", that is, two are shown with a short interval between, then there will follow a relatively long interval, after which two more syllables will follow, etc. I wish you to learn the syllables in pairs. The list will be shown five times.

Immediately after the fifth exposure I shall give you a typewritten list of the first syllables of the pairs and ask you to write the second syllables in the appropriate places. Do not begin to write until I have said, "Ready, write," and please say "Now" as soon as you have finished or are quite sure you can recall no more. Do not delay unduly, for I shall time you on this recall.

Then I shall give you a book from which I wish you to read aloud until I say "Stop." Just read, don't study—I shall not ask you to recall anything you have read.

At the stop signal I shall hand you a second list of the first syllables (arranged in different order from the first list) and again you are to write after each (or after as many of them as you can) the appropriate second syllables. I shall give you the "Ready, write" signal as before, and you will say "Now" when you have finished. (Time record taken here, too.)

Summary of the steps in the experiment.

1) Study syllables in pairs.

2) Write second syllables after appropriate first syllables on list. "Ready, write" signal and "Now" are important here.

3) Read until "Stop" signal is given.

4) Write second syllables after appropriate first syllables on a second list. "Ready, write" signal and "Now"—don't forget.

In addition to the written request not to discuss the experiment outside of the experimental room, the experimenter added a verbal request as the subject left the room.

In the preliminary practice period two lists of syllables were studied. First a list of five pairs was learned and recalled with all the information given that the subject might request. He was shown the typed list of first syllables prior to the exposure and was told what would be expected of him after the fifth exposure. With the second preliminary list of eight pairs, instructions were followed exactly. The subject read for two minutes from *Bill's School and Mine* after the first recall. The typed list of first syllables for the second recall was in reverse order from that of the first list.

At the regular experimental session the second week the subjects were given the same sheet of instructions, but were told that the lists would have six pairs and would be shown five times. After the

learning of the first list and the first recall of that list, the subject read aloud again the same passage from *Bill's School and Mine* that he had read in the practice session, and then was asked for the second recall.

A few minutes' interval was allowed between this recall and the presentation of the second list. Immediately after the presentation of the second list and the first recall of this, the experimenter started the stop-watch, then arose and went around the table, informing the subject that he would read as usual, but that there would be a diversion: "I'll put you in the electric chair for a while." The wristlets were then put on, and the book was handed to the subject. This required from 30 to 45 seconds as in the former experiment. The current was switched on at 30-second intervals as before, and the chair collapsed about seven seconds before time was called. Without stopping to remove the wristlets, the experimenter asked the subject for the second recall.

The following records were taken in this experiment:

1) The record of recall was taken in terms of second syllables supplied on the typed list of first syllables. The syllables were scored by the method suggested by Kjerstad (3), by which a possible 20 points is assigned to each syllable as follows: first letter of the syllable, 5 points; second letter, 3 points; third letter, 8 points; and position, 4 points.

2) A time record was kept for the recall of each syllable. This the experimenter obtained by making a time scale on a card upon which she could check the time (read from the stop-watch) as the subject completed each syllable. The paper upon which the subject wrote was always visible to the experimenter, but the subject was not aware of that fact. This method of timing as used in this experiment is admittedly not accurate, and the experimenter did not attempt to get the time closer than 2½-second intervals. Therefore all the records were given as 2½, 5, 7½, 10 seconds, etc.

3) A total time record was obtained as in Experiment 1, recording the time from the time the experimenter said "Ready, write," until the subject said "Now."

Results. With but few exceptions this group displayed the same aversion to the shock situation that had been evident in the group of graduate students. Both voluntary reports and the behavior of the subjects indicated this fact. The general disturbance was in most cases noticeable in the character of the reading. In the ma-

jority of cases the subject read as if he were just reading words, often making mistakes which rendered the material absolutely meaningless. The voice was characteristically heightened in pitch. When the shock was relatively weak, some of the men continued to read, but the voice went up the scale still more than the characteristic heightening mentioned above, and dropped again when the shock was ended. When the shock was stronger and the arms and the book were thrown upward, the reader stopped and often exclaimed and looked accusingly at the experimenter. Several laughed, one to such an extent that it made reading practically impossible. The fall of the chair was in many cases thought to be accidental. It did not seem so disturbing as the shock. Only one subject reported that it was more exciting.

In five cases the current failed to stimulate the subject for various reasons. However, since the bracelets were on in every case, and the fall occurred in all cases, the records of these subjects were not eliminated.

The recall data are presented in Table 4. In these figures the recall before interpolation in each control and test situation was taken as 100%, and the recall after interpolation is given on that basis. The "loss," then, is the difference between the average of the percentage recalled after interpolation and the recall before interpolation. This figure may, therefore, be read directly as the percentage of loss after interpolation.

In the control experiment the average loss was $8.83\% \pm 5.2$, while in the test experiment an average loss of $16.06\% \pm 6.6$ occurred. Neither of these losses is statistically reliable, but the loss in the test situation is somewhat greater than that in the control situation. A study of the individual records shows marked individual variations. Eight subjects made better records after interpolation than before in the control test, three made the same record in each recall, and 17 made poorer records. Compared with these performances, two subjects recalled more after interpolation in the test situation than before, eight made the same record before and after, and 18 made poorer records after interpolation. It appears, therefore, that about the same number of subjects showed a loss in both control and test experiments, but that those who lost, lost slightly more under the disturbing conditions.

The time records are presented in Table 5. These figures show the average time in seconds for the recall of each syllable written,

whether it was correct or incorrect, and the average total time elapsing from the experimenter's "Ready, write" to the subject's "Now." The time records for the incorrect syllables were included because the experimenter could not determine correctness or incorrectness of the syllables when this record was taken. Since N varies, it is given with each mean. Records of time for individual syllables were not taken for the first three subjects tested, hence even for Syllable 1,

TABLE 4

PERCENTAGE OF RECALL AFTER INTERPOLATION IN TERMS OF RECALL BEFORE INTERPOLATION (100%)

Subject	Control	Test
1	104.00	100.00
2	62.71	86.08
3	45.10	49.21
4	36.36	91.80
5	119.42	100.00
6	94.25	100.00
7	142.11	90.00
8	80.00	79.31
9	81.18	71.21
10	88.24	127.27
11	100.00	75.00
12	136.00	90.28
13	100.00	50.70
14	92.66	100.00
15	71.43	138.46
16	129.41	100.00
17	62.50	100.00
18	66.26	14.58
19	160.00	80.00
20	128.57	100.00
21	81.25	54.55
22	90.00	100.00
23	28.57	44.44
24	95.18	65.83
25	82.61	70.00
26	117.19	75.00
27	57.84	99.99
28	100.00	97.32
Average	91.17	83.94
$P.E._{av.}$	5.2	6.6
Loss (difference)	8.83	16.06
$P.E._{diff.}$	5.2	6.6
Difference (control and test)	8.23	
$P.E._{diff.}$	8.4	

TABLE 5

AVERAGE TIME IN SECONDS FOR EACH SYLLABLE AND AVERAGE TOTAL TIME

	Control				Test			
	Before		After		Before		After	
Syl.	Av.	*N*	Av.	*N*	Av.	*N*	Av.	*N*
1	4	26	4	26	4	26	8	26
2	7	24	7	23	6	26	8	24
3	8	20	11	18	10	24	8	21
4	8	17	11	15	5	20	1	17
5	7	10	8	9	6	14	14	10
6	13	10	9	8	16	13	7	8
Average total time	55	29	54	29	48	29	44	29
M. V.	21		24		21		20	

N is only 26, and it drops with each succeeding syllable as attempts to recall that syllable decreased in number.

The time required for the recall of the first two syllables seems to indicate a temporary interference of the emotional factor. The first syllable of all recalls, except that which followed the interpolated disturbance, required an average recall time of 4 seconds. After that interpolation the average for the 26 individuals was 8 seconds. For the recall of the second syllable 8 seconds were required after the emotional disturbance as compared with 6 and 7 seconds for the other three recalls of second syllables. An apparent retardation appears again in the case of Syllable 5; however, it will be noted that the number of subjects recalling that syllable was only 10, and the extreme score of one individual is responsible for the deviation.

Conclusion. As in the former experiments, recall records show a slightly greater, but not statistically reliable, loss in the test situation. Total time records show no evidence of emotional interference. The time for individual syllables recalled indicates a temporary emotional blocking, which passes away by the time the third syllable is recalled.

Experiment 3

Problem. This experiment was designed to supplement Experiment 2 in two ways: (1) a more accurate method of measuring the recall time for each syllable was utilized; and (2) data were collected from a larger group of subjects under conditions essentially the same as those of the former experiment.

Subjects. Forty-seven subjects were used. They may be divided into three groups with respect to their experience with the problem: (1) 9 subjects from Experiment 1, (2) 20 subjects from Experiment 2, and (3) 17 subjects who were entirely without experience and knew nothing of the problem, and one member of the faculty, who knew the problem, but had had no experience with the disturbing stimuli.

Method. The right associates method was used as in the preceding experiment. Six pairs of syllables were used in each list and were exposed five times.

The same records were taken as in the preceding experiment, i.e., correct recalls before and after interpolation, total time, and time for each syllable recalled, whether correct or incorrect. However, the timing apparatus was entirely changed, as described below. Scoring was done by the Kjerstad method. (A comparison of scores on the same data when scored by this method and by the arbitrary method used in Experiment 1 showed but slight differences in general results.)

The total time of recall, as well as the time of recall of each syllable, was recorded on a kymograph drum by means of two writing levers, one attached to a clock making contacts at one-second intervals, the other attached to a key operated by the experimenter. The subject wrote in sight of the experimenter as before. As the "Write" signal was given, the experimenter operated the key twice in rapid succession; the completion of each syllable by the subject was indicated by one touch on the key; while the subject's "Now" signal was recorded by three contacts. Time was then computed by comparing the two kymograph tracings. Interpolations were made to one-half a second.

The subjects who had served in Experiment 2 were given the following instructions.

Instructions will be the same as in the former experiment in which you served. They need only be summarized here:

1) Study the list of syllables which will be shown, associating the second syllable of each pair with the first syllable of that pair.

2) Fill in the second syllables of the pairs on the list of first syllables which I shall give you.

3) Read aloud until I say "Stop."

4) Fill in the second syllables on a second list which I shall give you. This procedure will then be repeated.

All other subjects were asked to read the instructions of Experiment 2 and were then shown a list of three pairs and asked for recall as a preliminary trial. The experiment proper followed immediately. It will be noted that this preliminary practice was less than that given the subjects in Experiment 2.

During the interpolated interval of the control test the subjects read aloud for two minutes from Wood's *The Influence of Monarchs*. At the beginning of the interpolated period of the test situation the electric wristlets were put on the subject, and he was handed the book with the instructions to read until told to stop. The current was turned on from three to six times, according to the reaction of the subject. The rheostat was always set at about half way for the first shock. If the subject reacted rather violently, the current was reduced, otherwise it was usually increased for each successive shock. Five seconds before the end of the period the gun was fired.

Results. The subjects' reactions to the disturbing stimuli were much the same as those described in Experiment 2. The shock was practically always effective in changing the character of the reading and always produced a sudden and extensive contraction of the arm muscles.

Two of the new subjects were very much concerned as to the knowledge and responsibility of the experimenter with respect to the strength of current employed. The experimenter did not answer questions, but asked if the subjects' hearts were all right! One subject from the graduate group became hysterical and cried at the second shock so that reading was impossible. Another graduate student gave a very violent reaction to the revolver report, jumping so that he bumped his knee painfully against the table. A number of subjects showed their emotional state by excessive perspiration of the hands, which, of course, enhanced the shock. This excessive moisture was noted by the experimenter when the wristlets were removed and was commented upon by the subjects.

The recall data are presented in Table 6. The group averages show a percentage of loss of 5.07 ± 3.4 in the control experiment and one of 19.24 ± 5.6 in the test experiment. The critical ratio is, then, 3.2, giving a possibility of 9850 chances in 10,000 of a true difference. The differences between the two losses is 14.17 ± 6.6, the critical ratio is 2.1, and the chances of a difference being greater than zero are 9200 in 10,000. These latter two differences, i.e., loss in the test experiment and the difference between the two losses,

TABLE 6

PERCENTAGE OF RECALL AFTER INTERPOLATION IN TERMS OF RECALL BEFORE INTERPOLATION (100%)

Subject	Control	Test
1	100.00	100.00
2	83.33	26.32
3	91.80	87.76
4	100.00	83.33
5	97.44	97.00
6	76.39	0.00
7	89.27	50.00
8	124.00	93.33
9	100.00	100.00
10	100.00	54.74
11	84.44	25.00
12	83.10	84.21
13	78.95	75.00
14	94.55	100.00
15	100.00	76.00
16	86.96	100.00
17	100.00	100.00
18	80.00	100.00
19	125.00	100.00
20	85.71	52.63
21	70.00	80.00
22	17.65	92.00
23	100.00	85.11
24	92.00	44.22
25	93.11	59.55
26	70.00	100.00
27	250.00	91.23
28	86.21	92.63
29	95.00	78.63
30	101.39	61.40
31	100.00	82.61
32	100.00	100.00
33	101.51	93.75
34	114.29	180.00
35	50.70	100.00
36	131.51	84.82
37	100.00	66.67
38	109.10	78.67
39	96.00	102.24
40	100.00	91.67
41	94.74	95.06
42	108.70	95.00
43	25.00	44.44
44	96.00	100.00
45	107.69	73.47
46	82.86	76.71
47	187.50	40.48
Average	94.93	80.76
$P.E._{av.}$	3.4	5.6
Loss (difference)	5.07	19.24
$P.E._{diff.}$	3.4	5.6
Difference (control and test)	14.17	
$P.E._{diff.}$	6.6	

are not statistically reliable, but are greater than have been found in any of the former experiments.

Individual records are again very variable, both in the control and test situations. In the control experiment 11 recall records were better after interpolation than before, 11 the same before and after, and 25 poorer after interpolation, while the corresponding figures for the test experiment are 2, 11, and 34, respectively. It appears that fewer subjects recalled more after the interpolation than before in the case of the test experiment than in the case of the control tests. Conversely, a greater number of subjects showed a loss after the interpolation of the test situations.

The fact was mentioned above that this experimental group might logically be divided into three groups: (1) a relatively experienced group, (2) a slightly practiced group, and (3) a naïve group. It may be interesting at this point to examine the records for these groups. The recall data are presented in Table 7. It is interesting to note that Group I shows a considerably greater loss than do either of the other two groups (25.09% greater than Group II and 22.12% greater than Group III). The subjects of this group, it will be remembered, had all gone through the disturbing situations a number of times and had become very strongly negatively conditioned to the whole situation. This greater loss is in contrast to the suggested practice effect mentioned in Experiment 1. An analysis of the time records for Group I show that the averages are approximately equal to those for the entire group as shown in Table 8.

Table 8 is in the same form as Table 5 which shows the time records for Experiment 2. The same phenomenon is shown here as was shown in that experiment. The time required for the recall of the first syllable was greater after the emotional interpolation than in any of the other three recalls. The difference between the time of recall of the first syllable before and after interpolation in the test situation is $4.8\% \pm .5$, a statistically reliable difference. This longer recall time applies to the first syllable only; the differences in the recall times for the other syllables, as well as in the total times, are not significant.

Unfortunately, we cannot correlate the degree of reaction with the effect upon retention, since we have no method of measuring the reaction. However, the records of the few individuals whose overt behavior was particularly striking are interesting with respect to time records. Subject 20 (Subject 9 in Experiment 1), who was

TABLE 7

PERCENTAGE OF LOSS IN RECALL FOR GROUPS I (EXPERIENCED), II (SLIGHTLY EXPERIENCED), AND III (NAÏVE)

Group	*N*	Control Percentage loss	Test Percentage loss
I	9	11.94	42.52
Difference		30.58	
II	20	8.29	13.78
Difference		5.49	
III	18	4.76	13.22
Difference		8.46	
Difference between Groups I and II*		25.09	
Difference between Groups I and III*		22.12	
Difference between Groups II and III*		2.97	

*These differences are those between the differences between control and test records given above.

TABLE 8

AVERAGE TIME IN SECONDS FOR EACH SYLLABLE AND AVERAGE TOTAL TIME

	Control				Test			
	Before		After		Before		After	
Syl.	Av.	*N*	Av.	*N*	Av.	*N*	Av.	*N*
1	3.8	44	3.9	46	2.5±.27	46	7.3±.43	46
2	4.4	43	7.6	44	6.7	46	6.0	45
3	7.5	39	4.0	38	4.4	45	4.3	41
4	7.3	34	8.5	30	7.2	44	5.6	37
5	3.9	27	3.1	29	6.0	32	2.4	26
6	4.5	12	8.1	14	6.8	16	6.9	15
Average total time	42.6	46	40.3	46	37.8	46	36.6	46
M. V.	17.4		21.1		13.4		14.8	

greatly disturbed, shows a recall time of 20 seconds for the first syllable after the disturbing interpolation, as compared with one second for the first syllable of the control recall. Subject 45, a member of Group II, also appearing quite disturbed, shows a time record of 17 seconds in the test situation, as compared with a control record of 2½ seconds. On the other hand, Subject 19 (Group 1), who became hysterical during the disturbing interpolation, recalled the first syllable in 5 seconds in both the control and test recalls. Subject 6 (Group I) has a record of 2 minutes in the test recall as compared with 2½ seconds in the control recall. This subject, it may be said, appeared very hurried and wrote six syllables in 14 seconds, but all were absolutely incorrect.

Summary and Conclusions

By way of summary, we point out the following facts which have been revealed in the experimental work just described:

1) A slightly greater average loss is found in all of the test experiments. It seems to the writer that this consistency is rather significant in pointing to some emotional interference.

2) A few individual records show striking losses where the emotional reactions were particularly marked. In one case complete forgetting occurred.

3) In the test situation of Experiment 3, in the record of the experienced group, who reacted very strongly to the disturbing stimuli, we find the greatest loss which occurred in the group of experiments. Unfortunately, because of the small number of subjects in this group, the difference between the control and test records is not statistically reliable, although it amounts to 30%.

4) Total time records in Experiment 1 show in 12 out of 15 comparisons a greater value in the test recalls than in the control recalls. The number of subjects was small, however, and the *M.V.*'s large. Hence, absolute values have no importance and we can again only point to the consistency of the results as evidence for an emotional effect upon recall.

5) The time per syllable correctly recalled in the recall test also shows a consistent tendency toward longer recall time in the test recalls (13 out of 15 comparisons). Again absolute values cannot be emphasized.

6) A phase of emotional interference which we might call emotional blocking was indicated in both experiments where the recall time of each syllable was recorded. In Experiment 2, 26 subjects required, on the average, twice as long to recall the first syllable after the disturbing interpolation as they did after the control interpolation, and in Experiment 3, 46 subjects required, on the average, approximately three times as much time for the test recall as for the control recall.

Opposed to the evidence just cited in favor of an emotional effect upon retention, we must emphasize the fact that all differences between control and test records are small and none of them is statistically reliable. Our results are not to be interpreted as proving that there is little or no loss in retention as a result of emotional excitement. It may very well be that such an effect is produced in a marked degree by the intense emotional shocks of everyday life, such

shocks as are involved in the well-known amnesias. Situations of this type could not well be employed in our experiments, with the result that the subjects may have compensated for the slight negative effects of the emotional upsets by an increase in effort to recall.

References

1. Cason, H. Specific serial learning: a study of backward association. *J. Exper. Psychol.*, 1926, **9**, 195-227.
2. Glaze, J. A. The association value of non-sense syllables. *J. Genet. Psychol.*, 1928, **35**, 255-269.
3. Kjerstad, C. L. The form of the learning curves for memory. *Psychol. Monog.*, 1919, **26**. Pp. 89.
4. Landis, C. Studies of emotional reaction: a preliminary study of facial expression. *J. Exper. Psychol.*, 1924, **7**, 325-341.
5. ————. Studies of emotional reaction: general behavior and facial expression. *J. Comp. Psychol.*, 1924, **4**, 447-501.
6. Robinson, E. S. Some factors in retroactive inhibition. *Psychol., Monog.*, 1920, **28**. Pp. 57.
7. Skaggs, E. B. Further studies in retroactive inhibition. *Psychol. Monog.*, 1925, **34**. Pp. 60.
8. ————. The concept of retroactive inhibition. *Psychol. Rev.*, 1926, **33**, 237-244.
9. Stratton, G. M. Retroactive hypermnesia and other emotional effects on memory. *Psychol. Rev.*, 1919, **26**, 474-486.

Clark University
Worcester, Massachusetts

L'EFFET DES RÉACTIONS ÉMOTIONNELLES SUR LA RÉTENTION

(Résumé)

Dans les expériences décrites dans cet article on a étudié l'influence des dérangements du corps sur la rétention. On a fait cinq expériences, 3 employant la méthode de rappel et 2 employant la méthode d'associations correctes.

Les 3 expériences avec la méthode de rappel n'ont pas indiqué d'une manière décisive la présence de l'interférence émotionnelle au point de vue des notations totales du rappel. Cependant, les moyennes des notations du rappel dans les situations de test on été un peu moins élevées dans toutes les expériences que les moyennes obtenues dans les conditions de contrôle. Ces différences n'ont pas été constantes d'une façon statistique, mais le seul fait de la tendance à la constance peut être significatif. Dans une de ces expériences où l'on a pris les notations de la durée totale du rappel, les chiffres n'ont pas montré l'effet de l'interférence. Une étude du temps qu'il a fallu pour rappeler chaque syllable d'une façon correcte dans cette expérience a indiqué quelque inhibition.

Deux expériences employant la méthode des associations correctes montrent la présence de l'interférence émotionnelle dans les notations du rappel

et le temps de rappel pour chaque syllable. Celles-ci montrent qu'il y a un empêchement temporaire du rappel après qu'on a interpolé les stimuli qui causent des dérangements. Cet empêchement disparaît bientôt, et la durée totale du rappel n'est pas plus grande dans la situation de test que dans celle de contrôle.

HARDEN

ÜBER DIE WIRKUNG VON GEFÜHLSREAKTIONEN AUF DAS DEDÄCHTNIS

(Referat)

In dieser Abhandlung wird die Wirkung von köperlichen Störungen auf das Gedächtnis erörtert. Man machte im ganzen 5 Experimente, 3 mit der Wiederholungsmethode, und 2 mit der Methode der richtigen Assoziationen.

Die 3 Experimente mit der Wiederholungsmethode bewiesen nicht endgültig dass Gefühlsstörungen stattfanden, was die totalen Wiederholungsleistungen anbetraf. Jedoch waren die Mittel der Wiederholungsleistungen in allen Experimenten etwas höher als die Mittel die man unter Kontrollumständen erhielt. Diese Unterschiede waren zwar nicht statistisch zuverlässig, doch die Tatsache dieser konsequenten Erscheinung an und für sich möchte von Bedeutung sein. In einem dieser Experimente, wobei man die Leistungswerte der ganzen Wiederholungszeit verzeichnet hatte, erwiesen die Zahlen keine Wirkung der Störungen. Eine eingehende Studie der Zeit für jede richtig wiederholte Sylbe deutete jedoch eine gewisse Hemmung an.

Zwei Experimente nach der Methode richtiger Assoziationen erweisen Gefühlsstörungen in den Wiederholungs-leistungen sowohl wie in der Zeit per Sylbe. Letztere Leistungswerte beweisen, dass nach der Einschaltung des störenden Reizes eine zeitweilige Hemmung der Wiederholung stattfindet. Diese Hemmung geht jedoch schnll vorüber, und die ganze Wiederholungszeit ist nicht länger bei dem Test als bei dem Kontrollexperiment.

HARDEN

TESTS FOR COLOR-BLINDNESS: A SURVEY OF THE LITERATURE WITH BIBLIOGRAPHY TO 1928*

From The Johns Hopkins University

ISTAR A. HAUPT

The anomalies of color vision present two aspects: (1) theoretical, and (2) practical, which have formed the basis for the development of the methods of testing for color-blindness and color deficiency. From a theoretical point of view it is important to discover exactly what the color-blind individual sees. We must know his sensitivity to color; the limits of his visible spectrum; the confusions that he makes; in fact, all his anomalous reactions to known stimuli. In this way alone is it possible to differentiate and classify visual function, thereby furnishing material for the construction of visual theories and concepts of the nature of color vision.

The only method of approach, in case of the congenitally color-blind, is by means of classification of the colors that the defective confuses or sees as alike. This method may be carried out by means of spectral light, picking out the regions which appear to the patient monochromatic; or by any one of the numerous tests which depend upon the ability to distinguish the color-tone of pigments presented in one form or another. So-called "classification tests" yield as data series of concomitant variations by means of which it is possible to arrive at a fairly complete analysis of the status of the defective's color vision as compared with that of the "normal" individual. A fairly extensive survey can be made of the responses to colors of different hues, saturations, and intensities; and of other factors that condition color vision.

The testing for color-blindness from the practical point of view derives its incentive from the use of red and green signal lights in the railroad and marine service, and the danger to public safety attendant upon the possible confusion of these two colors. We are not so much interested in determining whether the personnel of the railroad and marine service confuse pink, blue, or certain shades of tan,

*Received for publication by Knight Dunlap of the Editorial Board, March 19, 1928.

with green, etc., as in determining whether the men are able to recognize a standard signal promptly, at a safe distance, and to respond surely and correctly to its meaning. Inasmuch as colors and their names (the primary colors, at least) are associated early in childhood, the further association of red-danger and green-safety, etc., being developed later only according to professional needs, those who have as their task the elimination of the "unfit" from railroad and marine service have insisted most emphatically on the necessity of *naming* the color correctly, as well as distinguishing it from other colors, as an essential part of tests for color vision.

For the scientific investigation of the anomalies of color vision it is essential that the method of testing should give as extensive a survey as possible of the color response, and that the results should be expressed in absolute units, the significance of which does not change with time or place of examination. Undoubtedly, for this purpose, some form of spectral color-mixer, with adequate control of purity, intensity, saturation, etc., is the surest means of deriving results of the necessary degree of scientific precision. This method of investigation would not, however, be satisfactory for practical testing. The instruments are too expensive and require too careful handling; and the results are of significance only when interpreted by a trained investigator. Also, the time taken for examination renders this method unsuitable for testing great numbers of candidates for service.

For practical purposes, the chief requisite for a test for color-blindness is efficiency in picking out those unfit for service; the test must be diagnostic according to accepted standards of safety. There are, however, many other considerations that must be taken into account in devising a test for the practical examination of color vision. The apparatus must be simple in its manipulation, inexpensive, compact, and readily transportable. The results, too, must be simple, so that the average employee and applicant can understand the purpose and meaning of the test. It is also desirable that the task demand as nearly the same type of response as the applicant will be called upon to make in the exercise of his professional duties. These are but a few of the items of feasibility that have to be considered in the development and construction of the tests for the examination of color vision for practical purposes.

It is not my purpose to enter into the theories of color vision or

to discuss the attempts which have been made to explain the facts of color-blindness. A brief but adequate survey of the theoretical aspects of the problem has recently been made by Dr. Burton Chance (44). It is my purpose rather to trace the development of the tests that have been used heretofore, and the methods and standards of diagnosis of color-blindness.

It is useful to classify tests, as indicated by the following table, adopting a scheme which is perhaps somewhat arbitrary, but which has the virtue of being free from the influence of any color theory:

A. Confusion tests
    1) Classification tests
    2) Pseudo-Isochromatic tests
    3) Lantern tests
B. Contrast tests
    1) Flor-contrast
    2) Mirror contrast
    3) Colored shadows
C. Pupillary reaction tests
D. Rayleigh equation tests
E. Tests using special apparatus designed to meet the requirements of practical testing
F. Spectrum tests
G. Quantitative tests
    1) Intensity
    2) Saturation
    3) Visual angle
    4) Time of stimulation
    5) Time of reaction

The first extant description of a case of color-blindness is given in a letter from Joseph Huddart to Joseph Priestley in 1777 (335). Dalton's description of his own famous case was made to the Society of Manchester in 1798. There intervened, however, nearly 40 years before any attempt at a systematic investigation of the phenomenon occurred. Seebeck (287) was the first to describe a test for the detection of color-blindness. The task consisted in arranging according to the reciprocal resemblances some 300 different colored pieces of paper, glass, wools, etc. Silk was discarded because of its brilliancy. Seebeck was also among the first to state that there probably was a gradual transition in degree of defect from complete color-

blindness to normal chromatic sensitivity. The same method of examining for color-blindness, using various differently colored objects, was later used by G. Wilson (326).

A. Confusion Tests

In 1873 Holmgren (159, 160), in Sweden, took note of the need of an adequate test for the diagnosis of color-blindness among railroad employees. Instead of using varied materials he confined himself to the use of skeins of Berlin wools in various colors. His method differs from that of Seebeck and Wilson in that he furnishes a test skein and asks the testee to pick out from the lot of wools those skeins which resemble the test skein in color. A very complete description of the test and full directions in English are given by Joy Jeffries (169). The Holmgren test is well known and widely used, being the test most commonly used alone or with supplementary tests in Great Britain, Austria, France, Germany, the Netherlands, Norway, Sweden, Russia, and the United States (255). Greece and Italy have no official color tests. It is generally conceded that when the Holmgren test is properly applied by a competent examiner it has proved adequate and satisfactory in picking out those applicants whose color deficiency renders them unfit for service in the railroad and marine. Many modifications have been devised to meet the various objections that have been raised against it.

In order to prevent soiling and to facilitate the recording of the results of the test, Thompson (307, 308) suspended 40 skeins of wool from a stick, which could be presented to the testee in such a way that each skein was clearly visible. Each skein was numbered and the record kept in terms of the numbers on the skeins selected to match the test skeins. The stick could be folded and the wools protected with a covering. E. H. Cartright (42) describes "an improved wool stick for the testing of color vision." Forty skeins of wools of different hues are hung by small hooks along a stick. Three of them are the usual test skeins of pale green, rose pink, and red. The rest consist of matches and confusion colors for one or the other of these. The order in which the skeins are hung on the hooks may be varied at pleasure, but each skein has a number attached to it and the numbers are so arranged that all the odd numbers are matches and the even ones confusion hues. The testee is given a test skein and told to pick out all the other shades of the

same color. To the back of the stick is attached a sheet of white twill, which, hanging down behind the wools, forms a most suitable background for the colors. The stick folds in two on a hinge and the skeins and twill sheet can be wrapped around it, the sheet effectively preventing any tangling when the wools are again unwound for use. The stick is not intended entirely to supplant the Holmgren wools but has been found adequate in a large majority of cases.

Schenkle (281) uses 40 or 50 rods, 12 cms. long, wound with wool, which can be put into a narrow wooden ring making a colored star. He uses the vertical radii as test colors and lets the patient remove all the rods which do not seem related. The rods are removed until only those of one color, in different tints and shades, remain. Dennet (62) has put up the wools in the form of spheres, such as are used in upholstery trimming, 1 inch in diameter. All the balls are put on the table and the testee asked to sort them into three trays, pink, light green, and purple. Dennet claims as advantages for this type of presentation that (1) all the test objects are of the same size; (2) all are seen at once; (3) all are equally illuminated; and (4) all are tagged according to Thompson's suggestion. Jeaffreson (168) has devised a rotating celluloid disk, 1 foot in diameter, upon which skeins of wool are arranged radially at the outer edge. All of the disk, except a small aperture, is covered. By means of a button attached to the center of the disk, it can be turned until any color is brought opposite to the standard test color which is seen in the upper half of the aperture. The test colors are the three Holmgren test colors and a yellow, blue, and purple. The test color is exposed and the patient asked to turn the disk and point out the colors on the disk which look like the test color. The record is kept in terms of the numbers attached to the colors on the disk. A modification of this scheme has recently been used by Nelson (238), using either wools or painted surfaces. Hoffman (158) has modified the form of presentation of the Holmgren wools by wrapping them around corks. He uses 228 colors and the same test colors as the original Holmgren. The patient is asked to point out all the colors which seem to go with the test color. Armaignac (7) takes great pains in selecting the colors of his wools. He matches them to the Gobelin samples representing the spectrum colors. He had the wools twisted into 6 cm. cords and made into tassels, furnished at the top with a cord, and placed in a box so that all could be shown at once. First

the box was placed end up, exposing only a small surface to detect central defect. Then the entire tassel was shown. Armaignac does not describe his method very explicitly, merely saying that the testee was expected "to recognize the color." He asked for names of the colors as a check on color ignorance. Sewall (288) has merely a sample card of wools, 18 columns of colors, each represented by from 5 to 15 tints in graded series and grays of different brightnesses. He chooses a light green as test color and, pointing first to the grays, asks the testee to pick out the one that "exactly resembles" the test green. If the patient accepts a gray as exactly matching the test color, Sewall proceeds to ask him to pick out from the colored series all those which match the test green. The same procedure is followed with the pink test samples, etc.

The most recent modification of the original Holmgren method has been devised by Jennings (170-176). The test consists of a square box, the upper and lower half each having a lid. The top half is Test I, green, and contains a color board made up of all the different colors, shades, and tints likely to be mistaken by the color-blind patient for the test green. The lower half contains Test II, rose. Each color board contains 64 patches of worsteds of various colors and tints and shades. In each patch is a circular opening for making a pencil mark on the record paper underneath. The arrangement of the patches is symmetrical so that any side may be presented. The record for the two tests may be made on the same sheet. Jennings' directions for giving the test are as follows: A green test skein fastened to the inside of the box cover is placed at a distance of two feet from the patient, who is given a pencil and requested to look along each row of colors and when he sees the test color, or one of its lighter or darker shades, he is to place the point of the pencil in the hole and make a mark on the record sheet beneath. As advantages over the original Holmgren test Jennings enumerates the following:

1) The candidate makes a permanent record of his own test.
2) The worsteds are not so easily soiled.
3) Two groups of colors are used, red and green.
4) Comparison of colors by placing two or more together is avoided.
5) A large number of colors may be presented.
6) The size of the patch makes it useful as a test for central defect.

7) Easy recording.
8) Danger of losing an important color is obviated.
9) Symmetrical construction.
10) Two or more records can be made at one sitting.

The new edition (175) of the test eliminates the stone colored and all shades of violet as they proved to be not diagnostic and confused the interpretation of the results.

A modification of the Holmgren method using material other than wool was introduced by Adler (3), who suggested the use of crayons. These are not subject to fading and soiling, and a permanent record of the test can be kept. He presents the testee with a number of crayons placed point up in a cup. The examiner takes out a light green crayon and asks the testee to pick out all those crayons which appear to him to come nearest to this sample in color. He is asked to make a check or cross or write a word with each crayon as he selects it from the cup. This constitutes the record of the testee's choice of colors to match the test color. The same procedure is followed with the red and purple test colors. Goertz (90) introduces a further check by requesting the testee to write the name of the color he has selected on the record sheet. If the patient is illiterate, he is asked the name, and the examiner writes the name at the testee's dictation, making a note to that effect. Vierling (315) is the most recent exponent of this method. He suggests that the crayons be selected with reference to the confusion colors.

Badal (11, 79) used 20 cylinders to be fitted into holes in a plate according to their color. Cohn suggests the use of powders instead of wools. He presents the testee with a number of small glass containers filled with different colored powders and requests the patient to identify and name them.

Although the method of testing for color-blindness by means of free selection and identification of colors has been applied extensively from the time of Seebeck on, it was not long before it was observed that color-blind persons make very typical confusions in the selection of similar colors. This discovery led to the development of a great number of tests based on the principle of confusion colors, which may be grouped under the head of pseudo-isochromatic tests.

Daae (61), in 1878, arranged his wools in squares on cardboard. He had 70 colored surfaces, each made up of strands of wool, 1 cm. long. These were arranged in 10 horizontal rows, graded from

light to dark. One row contained only green in various tints and shades, another only red, others mixed and pseudo-isochromatic. The testee is requested to point out the rows which contain only one color, the isochromatic rows. Keersmacker (177, 178) arranges his wools in strips and requests the testee to measure with a pair of dividers the width of the strip that appears to him to be isochromatic. Reuss (263) describes his *Wolltäfelchen* as follows: There are 60 cards made of gray paper, 7.5 cms. wide; 5 cms. long. On each are two colored squares, 14 mm. each, made up of 7 wool threads. Eight cards have the two squares identical, namely, one card each of red, yellow, green, violet, purple, and three blue cards. Six cards have isochromatic but not identical squares, namely, one each of yellow, blue, violet, purple, and two green cards. The remaining cards have pseudo-isochromatic squares arranged as follows:

To correspond with Holmgren's Green Test
- 4 light greens with browns
- 2 light greens with grays
- 1 light green with purple
- 2 dark greens with brown
- 2 dark greens with gray

To correspond with the Purple Test
- 2 medium purple with blue
- 2 medium purple with violet
- 4 medium purple with blue green
- 4 medium purple with gray

To correspond with the Red Test
- 3 red with green
- 6 red with green-brown and brown

For examining weak color sense
- 1 light yellow-green with light yellow
- 1 light pink with lightest gray

For a blue-yellow blind test
- 5 blue with green
- 1 blue with gray
- 1 blue-green with violet
- 2 purple with red
- 2 purple with yellow
- 1 gray with yellow-brown

The testee is asked to sort the cards on the basis of similarity and dissimilarity of the colored squares.

The Pocket Test of Edridge-Green (101) consists of 112 single threads of wool, 13 pieces of twisted silk, 9 small pieces of cardboard, 3 pieces of changeable silk ribbon, and 1 piece of green velvet. The colors are selected and so arranged as to confuse the color-blind individual. The testee is asked to pick out all the shades of the color similar to I, etc. The Edridge-Green Classification Test (93, 102, 112) is an elaboration of the same method.

Donders (210) has pairs of confusion-colored wool rolled on a stick, one color forming the ground and the other a strip over it. The testee is required to count the number of strips. Holmgren (210) and Stilling (210) have devised pseudo-isochromatic color fields resembling Ruess' cards. Cohn (50) embroidered patterns with wools using pseudo-ischromatic colors. Burch (31, 32, 33) painted letters and phrases on painted Academy Board. He selected his colors after very careful study of the confusions made by a red-blind person. He added the further device of a perforated zinc screen placed at the focus of a convex lens in front of the test cards. When one looks through the screen at the test cards a mosaic of colored dots is seen, disguising the brush strokes made in painting the patterns. The advantage of the painted test cards is that they can easily be varied in design and the colors kept fresh. Podesta (252) used a similar device, adding the feature of having one word in pseudo-isochromatic colors and a second word in the same color but darker than the background and visible to normal and deficient both.

Mauthner (210) soon met with the difficulty of keeping wools and papers clean and bright and suggested the use of pseudo-isochromatic powders. He had a number of small test tubes filled half and half with carefully selected pseudo-isochromatic and isochromatic powders. He found the manipulation of this type of material easy and the results satisfactory.

Hembolt (138, 139, 255) uses pigment papers in the form of small disks, 6 mm. in diameter, arranged 3 mm. apart and 3 mm. from the edge around the periphery of two metal disks, 18 cms. in diameter. Each metal disk presents 58 confusion colors. The colors are presented, each paired with every other, through an opening in the screen. The patient is placed 1 m. from the screen with his back to the window and is asked to tell when the colors are the same and when different. First a red on one disk is shown with all the colors on the other disk; then a green, etc.

The best known and most widely used form of pseudo-isochromatic tests for color-blindness are those of Stilling (295, 296), Grossman (119), Nagel (231), Oguchi (240), Ishihara (166), and Edridge-Green (110). They consist of cards with patterns, dots, circles, irregularly shaped spots, etc., printed in colors on a colored background. A design or letter or number is worked in one color against a pseudo-isochromatic background. It is important to arrange the design so that the patient is forced to judge by color perception alone. It is possible in constructing these cards to select color combinations that will reveal different types of defect. This device for testing color perception has been used extensively, though many questions have been raised as to its reliability. The greatest difficulty seems to be in getting an exact reproduction of the colors in printing the cards. The Stilling cards have been considered rather more satisfactory than the Nagel cards. Gildemeister (89) uses the Stilling cards, projecting them across the room on a screen in order to eliminate the glare.

Nagel gives the following directions in the 1906 edition of his cards: I. Show me a plate on which there are red or reddish dots. II. Show me a plate on which there are only red dots. III. Same as (I) using green and gray. In the second part of the test, displaying a card with green and brown, he asks, "What colors do you see here?" The same question is asked using the other cards, red and brown, etc. The Oguchi and Ishihara test plates are very much alike, both in design and color. Edridge-Green has incorporated a few minor improvements in his cards, making the test more sensitive and the diagnosis of the type of defect (according to his classification) more obvious. It has also been suggested (215) that the cards be so designed that the figures are apparent to the color-blind but not to the normal individual.

A further development of tests for color vision on the basis of the typical confusions of color made by the color-blind arose from the need of selecting men for railroad and marine service who would be able to recognize the signal lights with requisite speed and accuracy. It was argued that in all fairness the applicants should be tested at tasks which reproduced as closely as might be the conditions they would have to meet in the fulfilment of their duties in the service. An engineer must be able to tell a red signal from a green one at a safe distance and act accordingly. In

order to meet this phase of practical application of tests for color vision, various and sundry modifications of signal lanterns have been devised for test purposes.

Among the first advocates of lantern tests was Donders (66), in Holland. He used a candle behind a screen which had a ground glass opening 25 mm. in diameter. A metal disk with holes 1, 2, 5, 10, and 20 mm. in diameter arranged around the periphery was adjusted in front of the opening. Behind this disk was another rotating disk with holes filled with glass of different colors. One hole was left clear. The candle is moved along a scale, thus giving known variations in intensity. By means of this arrangement, Donders was able to get a quantitative estimate of color perception on the basis both of intensity and of visual angle. He insists that for railroad service it is important that the candidate be able to name the colors, although he also asks the candidate to match the test light with colored samples of wools, papers, etc. Brailey (25) recommended the use of the Donders Lantern as a test for sailors in the English service. Buxton and Edridge-Green have devised modifications of Donders' Lantern which have found very extensive application in England. Buxton's (36, 37) "Instrument consists essentially of a lamp contained in a light tight lantern, having in it a circular aperture one inch in diameter. About two inches in front of this aperture is a metal screen six inches square, also pierced with a corresponding hole of the same size as that in the lantern. Between the screen and the lantern (and attached to the former) is a brass carrier holding a red and green glass and so arranged that either may at will be placed in the field by a slight movement of the finger of the operator. In front of the screen is a metal disk with a number of holes of different sizes and disposed at equal distances near its margin. On rotating the disk the holes are brought one after another opposite the illuminated area. . . . The red and green glasses are changed about each time at the operator's pleasure. One segment of the disk is devoted to another phase of the test and is perforated with holes in various relations to each other. They are supposed to represent tail lights of a distant train. The examinee must be able not only to see their color but also tell their number and positions. Finally the second disk is placed in front of the first and is fitted with a series of neutral tinted glasses of different intensities to represent the

various degrees of fog" (255). Some years after the invention of this apparatus Buxton combined the lantern test with a matching test (37). The lantern is placed 15 feet away from the candidate, who himself remains in daylight with a set of Holmgren wools on a table beside him. With varying intensity of light in the lantern, the candidate is required to pick out skeins of wool which match each of the telechrome colors, irrespective of shade.

Baldwin describes a somewhat crude apparatus operated on the same principle as the lantern tests. It consists of a flat box, open at one end. Both sides have a circular opening, 1½ inches in diameter, and are grooved along the top and bottom to allow the insertion of colored glasses and a wooden slide containing a diaphragm with a range of opening from 1½ inches to ½ inch in diameter. The box is held up before an ordinary Argand burner; the testee stands 4 yards away. Using the smallest opening of the diaphragm, the lantern represents a 6-inch signal light at 288 yards. The largest opening represents such a signal as seen at 16 yards (13,14).

Edridge-Green's lantern (106, 112) is based in principle and in construction more directly on the original Donders' Lantern. He uses 12 slides, 6 colored and 6 modifying glasses. The testee is seated at a distance of 15 feet and asked to name the colors as they are shown, first alone and then with the modifying glasses. Dr. Green makes rather a point of his care in selecting the smoked glasses, so that their transmission shall be as unselective as to wave-length as possible. He submits both his colored and his modifying glasses to spectroscopic analysis. The most recent form of the lantern (112) devised by Edridge-Green, which is now extensively used in Great Britain by the Boards of Trade, consists of a lantern and four disks, three colored and one with modifying glasses. The colored glasses are: (1) standard red A, (2) standard red B, (3) yellow, (4) green, (5) signal green, (6) blue, (7) purple, and (8) clear. The modifying glasses are (1) ground glass, (2) ribbed glass, (3) clear, and (4) four different densities of neutral smoked glasses. The ground glass represents mist; the ribbed glass, rain; and the neutral glasses, fogs of different densities. There is also a diaphragm to represent a 5½-inch railway signal at 600, 800, 1000 yards, when the test is conducted at 20 feet. Each color on the disk is shown, and the candidate is asked to name the color

of each; occasionally he is asked to match the lights with a selection of colored wools, silks, cards, etc., from Dr. Green's Classification Test. Edridge-Green states very emphatically that color ignorance is sufficient cause for rejection from service.

Williams introduced the use of Donders' Lantern in this country. His modification consists of a lantern provided with a disk having 15 colors and a set of diaphragms, the smallest of which represents, at 20 feet, a signal-light lens at 1500 feet. In the earlier model the variation in intensity was effected by means of smoked glasses; later, with the use of incandescent lamps, the variation is accomplished by changing the resistance of the lamp (324).

The lantern devised by Everbusch (75), which is very extensively used on the continent, is also based on Donders' principle. The construction is very neat and the operation quite simple. An attachment reproducing daylight signal conditions is provided, making the apparatus a very complete practical test. Erdmann (74), in his modification, has added more colors to the Donders and Everbusch lanterns.

Kennet-Scott (179) and Mortimer Frank (79) have described the lanterns they use in the examination of candidates, but have given little indication of the exact procedure, whether naming or identification is required of the testee. Dunn's "Color test appliance" (70) is in the form of a screen which is placed in front of a source of light. The accepted devices for presenting the colors and for controlling the intensity and size of the stimulus are provided. The testee is given a card with the four primary colors on it; he must name the colored light shown through the screen and then identify it on the card. The most recent description of a lantern in this country is given by Jennings (174).

B. Contrast Tests

The phenomenon of color contrast, both simultaneous and successive, has furnished a basis for tests for color vision. The tissue-paper experiments of Meyer served as a starting-point, though it was not until some 25 years later that contrast phenomena were used as tests for color perception. The special test devices for the detection of color-blindness may be divided into three groups: (1) the so-called Flor-contrast, (2) mirror contrast, (3) colored shadows. The first of these is a direct descendant from the original experiments of Meyer (217). Pflüger (250, 251) used black and

gray numbers and letters printed on a colored background. The test cards were covered with from one to three thicknesses of tissue paper. The normal eye sees the letters and figures through the tissue paper colored with the complementary color, whereas to the color-blind person, they are invisible. The grays are so selected with reference to the brightness of the colored background that there is no brightness contrast. Some cards, with the black letters, are especially designed to reveal simulants. Cohn's test (51) consists of 16 *E*'s in four rows on a colored ground, covered with tissue paper. Whoever sees the letters through the tissue paper has fine color perception. The testee is further asked to indicate the direction of the *E*'s. To discover simulants, Cohn uses the scheme of having the testee look at the test plates through a red glass; both normals and defectives should thus see the letters through the tissue paper.

Mirror contrast was first described by Brandes, and later by Ossan and Dove.[1] Mauthner (210) describes Ragona Scina's device as a test for color perception, which has also been used by Pflüger and Cohn. A sheet of white writing paper is spread out and pasted on thin cardboard, folding in the crease of the paper, so that the two halves are held at right angles to each other. A piece of dull black paper or black velvet, 20 mm. square, is pasted 20 mm. from the crease on the horizontal half of the paper, and a similar patch is pasted 30 mm. from the crease on the vertical half of the sheet. The observer looks down on the black patch on the horizontal half through a red glass held inclined at 45° between the two halves of the writing paper. He sees the vertical patch illuminated by light reflected from the horizontal surface through the red glass, colored red, and the horizontal patch illuminated by the white light from the vertical paper reflected to the eye directly from the surface of the glass, tinged with the complementary color.[2]

Colored shadows thrown on a screen by interposing an opaque object (rod, pencil, etc.) in the paths of two sources of light, one colored and the other white, produce very pure saturated com-

[1]Helmholtz, Physiologische Optik, 1st ed., page 417.

[2]Thomson and Weiland (309) also refer to Helmholtz' leukoscope as a test based on contrast, but we have been unable to find a description of this instrument.

plementary colors, which can easily be controlled in brightness. If the colored light be red, its shadow, illuminated by the white light, will be brilliantly colored with the green complementary to the red. This phenomenon was observed by Leonardo da Vinci and applied by Stilling (210) as a test for color-blindness. The same principle underlies Cohn's "Chromoskiometer" (169), Holmgren's instrument (169), and Waldstein's "Chromatoscope" (169), and recently has been used by Grill (117). The task in all of these tests is based on the testee's ability to see the contrast colors. Meyer, Weber, Pflüger, and Cohn recommend the identification of colors as well as the naming.

C. Pupillary Reaction Tests

Moritz Sachs (144, 278) applied the reaction of the pupil of the eye to intensity of light to the problem of testing for color-blindness. On the basis of the Hering theory, a colored light has two achromatic components conditioning its intensity value: (1) the white valence, or the amount of white light mixed with the color, and (2) the specific brightness of the color which is inseparably bound with the chromatic component of the light. Primarily, the reaction of the pupil depends on the intensity of the light; but for the color-blind, who have lost the chromatic component of the light, the intensity is reduced by an amount equal to the specific brightness of the color. It follows that the color-blind person reacts with his pupil only to the white valence. Sachs proceeded as follows: In the closed shutter of a window was placed an opening 80 cms. square. A frame moved in slides before this, having two colored glasses of the same dimensions as the opening. The testee sat ½ m. from the window and fixated the center of a window cross, attached to the opening, his head resting in a head-rest. The pupil was observed with a telescope with a 2 magnification. The edge of the iris was illuminated, when necessary, with a Wolz microscope lamp. A dark yellow-green glass and a light yellow-red glass were inserted in the frame for a color-blind person, who finds the red a trifle lighter than the green. For the normal the red is much lighter than the green. The color-blind fails to respond with the pupil. Sachs concludes, ". . . the play of the pupil is different for the color-blind patients than for normals, and the deviations are comprehensible on the basis of the changes which the lights undergo for

the color-blind, so that under certain conditions it is possible to deduce the type of color experience from the observations of the pupil" (translated from p. 126). Some years later Abelsdorf (1), following the same procedure, found the following results:

1) red and green, red being lighter than green;
2) red and yellow, red being lighter than yellow;
3) yellow and blue, yellow being lighter than blue.

He finds that for the normal the pupil is

1) smaller for red than for green;
2) smaller for red than for yellow;
3) smaller for yellow than for blue.

For a 17-year-old red-blind lithographer, the pupil was

1) smaller for green than for red;
2) smaller for yellow than for red;
3) smaller for yellow than for blue.

A green-blind case reacted the same as a normal (Prof. Brodhun).

A total color-blind case reacted as a red-blind.

An hysterical color-blind patient reacted normally.

The most recent study of the pupillary reactions of the color-blind has been made by Engelking (71, 72), Groethusen (118), and Hess (147).

D. Rayleigh Equation Tests

Another principle of testing for color perception is based on the observations, originally made by Maxwell (212, 214) with pigment papers, that it is possible to match red and green to match a yellow. Lord Rayleigh (259, 260) applied this principle in devising a test for color-blindness. Using spectral lights furnished by a double refracting prism, he found the relative proportions and intensities of red and green lights, which, when mixed, would match a yellow light, both in brightness and in hue. This has come to be known as the "Rayleigh equation." Among 23 male observers whom Lord Rayleigh had asked to establish this equation with his apparatus, he found 7 who disagreed with his setting, but agreed among themselves according to their type of defect. Seven female observers were satisfied with Lord Rayleigh's setting.

Instead of using double refraction prisms to obtain his two spectra, Nagel (227, 233) used a double-slit arrangement in the construction of the Anomaloscope. The slit (S_1) regulated by the screw

(G_1) gives a simple spectrum in the plane of the objective slit. The two slits (S_2) and (S_3) are coupled so that one can be widened only at the expense of narrowing the other. When these slits are both open, there are formed two partially overlapping spectra in the plane of the objective slit, lying above the simple spectrum from (S_1). Near the prism end of the objective tube is a twin prism with horizontal refracting edge, in front of which is a circular diaphragm. The observer sees through the objective slit a circular field limited by the diaphragm which is bisected by a horizontal line, namely, the twin prism. The lower half of the field is filled with light from (S_1) and the upper half with the mixed light from (S_2) and (S_3). A small convex lens is set in front of all three slits so that the rays from all three are derived from one point in the source. A Nernst filament with a matt glass in front of it is used as a source, placed 18 cms. from the slits. Nagel gives the settings of the screws controlling the slits which give the equations characteristic of normals, deuteronopes, and protonopes. These settings must be found for each instrument by calibration. Nagel states that the normal can achieve a mixture of green and red which will match the yellow. By regulating the brightness of the yellow, the match can be made perfect. Similarly, the red-anomalous person and the green-anomalous person can find a certain red-green combination that matches a certain yellow brightness. Deuteronopes differ from protonopes in requiring different brightnesses of yellow. A later model of the Anomaloscope has a movable objective arm, making it possible to establish equations between other colors. Hegener (136) has attempted to devise an apparatus on the same principle as the Anomaloscope, but with simpler construction and larger fields. Hess (148), too, has objected to the clumsiness and troublesomeness of the Nagel apparatus and has gone back to the original Maxwell experiments, using pigment papers on a color wheel. He uses paper disks, red and green to give a gray and an outside ring of black and white to match the gray from the mixture. Hess claims that you can get a quantitative estimate of the underestimation and overestimation of the colors by color-blind persons from the proportions which they require to achieve the matches.

The Hering Color-Blindness Apparatus (143) is constructed on the basis of the Rayleigh equation of testing color perception. Cowdrick and Winfield (60) give a very complete description of the

apparatus. "At the bottom of a short vertical tube is a circular field which consists of two semicircular fields, red and green. The former is furnished by a window filled with red glass set in the floor of a light chamber and illuminated by a white light from the surface of a reflector. The reflector moves through a horizontal axis through 120° and its position, and therefore the amount of reflected light, is marked by a pointer which travels over a graduated arc. When the pointer is at '0' the red is at '0'; when the pointer is at 120°, the red is at maximum. The hue of the red, which is slightly orange, may be changed by the admixture of blue. This blue light comes from another window filled with a blue glass set in the left side of the light chamber and illuminated by a reflector which moves through a vertical axis of 120°. The light after passing through the blue glass is reflected into the eye of the observer from the surface of clear glass set at an angle of 45°. Since the red rays pass through the same clear glass they may be mixed with the blue in variable amounts, so that it is possible to obtain all hues from yellowish red to violet. The green stimulus comes from a window set in the right of the light chamber and illuminated by a reflector in every way similar to that of the blue; its light is reflected at an angle of 45°. The task of the observer is to match the two semicircular fields in brightness and hue. The match for tint is made by changing the intensity of the green, and that of hue by changing the intensity of the blue. The intensity of the red is fixed by the experimenter at the beginning of the test and is not altered throughout its course." Ives's new duplex diffraction spectroscope (262) has been described by the Committee on the Standards and Methods for the Examination of Color Vision. It is used as a spectro-color-mixer to get the Rayleigh equation. Louis Bell (262), too, uses his spectrum device to get Rayleigh equations.

Rose's polariscope (272, 273, 274) consists of a polarization apparatus, i.e., two Nicol prisms with divided circles, having a fixed achromatic double refraction prism and a crystal plate ½ cm. thick, ground perpendicularly to its axis. Through this arrangement is seen the image of a diaphragm, always double, contiguous, with each image colored complementarily. The color depends on the position of the upper Nicol and the intensity on that of the lower Nicol. It was found that normals and completely color-blind individuals cannot get a match between the two complementary fields,

but the color defective can do so easily by regulating the color and intensity.

Weiland has devised an attachment for Javal's keratometer that transforms this instrument into a test for color-blindness (319). The attachment consists of a straight metal tube 1½ inches in diameter that reaches from the place where the patient's cornea usually is to about the beginning of the telescope tube and is so fastened to the head-rest that its axis coincides with the axis of the instrument. At the front of this color tube there is a plain glass plate behind which is a Nicol prism fastened in a cork. From this prism the polarized light passes by a round diaphragm through a quartz plate cut at right angles to its axis, about 5 mm. thick. The observer sees two fine large color fields, partially overlapping, complementary in color, with the overlapping area white. If the arc of the Javal is rotated, the colors will change, but always remain complementary. The testee is placed with his eye at the ocular of the instrument. The Nicol is set for blue and yellow. The patient is asked whether the colors are exactly alike or shades of the same color. The Nicol is then turned through an angle of 90°, the testee being told to stop as soon as the two colors are the same. Normal persons never see the colors alike. By putting a second Nicol in front, the brightness can be varied until for the color-blind person the match between the complementaries can be made perfect. To catch simulants, Weiland suggests having the testee look through a red glass. At some position in the arc the two fields must appear to be alike. This attachment to the keratometer is, of course, nothing more than the original polariscope of Rose fitted on to the instrument of Javal. Weiland also gives a reference to an Ophthalmo-leukoscope of König, but we have been unable to find a description of that instrument in the literature. The *Polarizationsanomalaskopet* of Göthlin is a device based on the same principle (91).

E. Tests Using Special Apparatus Designed to Meet the Requirments of Practical Testing

Black (20) uses semaphore figures in black on a white ground, 5 min. visual angle at 20 feet equivalent to semaphores 46 in. long at 2600 feet. He also uses red and green disks. The testee is required to name whether the signal is clear, caution, or danger.

No color names are used. He also suggests using three boxes, clear, caution, and danger, into which the Holmgren wools are to be sorted.

Lediard (195) and Scripture (286) have arranged adaptations of pocket ophthalmoscopes as tests for color vision. Lediard has inserted purple, mauve, blue, green, and yellow glasses in the space where the lenses usually are, but he gives no description of the way he uses the tests. Scripture describes his device as follows: The first model has three windows of glass, 8 mm. in diameter, numbered (1), (2), (3). The opposite side of the tester consists of a movable disk carrying 12 glasses of different colors. As the disk is turned various colors appear behind the three windows. The subject calls off the colors seen at the windows. Window No. 1 carries a very dark smoked glass; all the colors seen through it are seen very dark. No. 2 has nothing and No. 3 has a light smoked glass. The tester is held in front of a window or some white surface at about 5 meters from the person tested. The operator begins with any chance position of the glasses and asks the testee to tell the colors seen through the three glasses in 1-2-3 order. The operator records the letters on the back of the tester to indicate what colors were used. A slide with different sized openings is also provided. The second model has a greater number of colors. In addition to these there is a disk with ten windows of glass of the colors used for signalling. These colors are carefully matched and tested with the spectroscope. The disk is numbered along its edge. Another disk carries smoked glasses, and two diaphragms are attached.

One of the most widely used appliances designed to test color vision is the *Farbengleichungsapparat* of Nagel. This consists of a lamp with two metal disks with colored glasses which can be rotated in front of the lamp, so that two contiguous fields on a black ground can each be illuminated with red, yellow, green, blue, or white. Two slides permit independent variation of intensity. The testee is put 1½ to 2 meters in front of the apparatus and is shown (1) one side bright red and the other dark red; (2) both sides the same bright red; (3) one side yellow and the other red. For (1) the normal and anomalous color-blind see two degrees of brightness of the same color. The color-blind usually get confused and call one green or yellow. (2) is given to illustrate what the

colors look like when they actually are of the same hue and brightness. For (3) the color-blind has a certain brightness of yellow which equals the red. Slide No. 1 is made to give the proper brightness for protonopes. Slide No. 2 is made for the deuteronopes. The chief objection to this apparatus is that the settings are so fixed as to pick out only one degree each of the protonope and the deuteronope, and fails to give a clear diagnosis when the testee is not content with either of the two intensities presented. Vierling (314) uses colored gelatines instead of glasses and gets new settings, but otherwise contributes nothing new.

"A test for central color perception" has been described by Fridenberg (81). Test objects, 4 mm. square, are cut out of variously colored cardboard or paper. These are held in the palm of the left hand, hidden from the patient's view. With a small forceps held between the thumb and third finger of the right hand, one of these colored squares is picked up and the index finger extended over it so that it is still covered when the hand is held up before the patient, who is told to look sharply at the tip of the index finger. When convinced that the fixation is absolute, the target is exposed for a moment. An improvement of this scheme is in the form of an ophthalmoscope. A variable aperture is uncovered by a slide which springs back into place like a shutter on a camera, making as short an exposure as may be desirable. A rather better device for testing central color vision is in the form of Oliver's modification of the Abney Pellet Test (242). This consists of a black disk and ivory pellets 4 mm. in diameter, definitely gauged in their relative tinting to equal degrees of saturation. The test is placed on a large black table, and one eye used at a time. With a forceps a green pellet is taken out of the disk by the examiner. The task is to pick out the pellets which match the test pellet. The patient is requested to pick up the pellets and hold them in a position before him at which they appear to him brightest and plainest. If he turns his head to see more clearly or holds the pellet eccentrically, a central defect is revealed. He is asked to hold his head in such a position that his exposed eye is situated at some 40 to 50 cm. directly above the color tray. After the green test color, red, blue, and yellow are used in the same way. Edridge-Green (112) has described a bead test which is practically the same as Oliver's test.

F. Spectrum Tests

From a scientific point of view, analysis of color perception should undoubtedly be made with spectrum light. By this means only can a true comparison be made from laboratory to laboratory or even from observer to observer in equivalent terms. With a spectroscopic apparatus it is possible to specify intensity and hue in absolute terms. The examination should include a determination of the limits of the visible spectrum, of hue sensitivity, of the position and extent of neutral bands, of chromatic sensitivity at some of the more important points in the spectrum, as well as a study of the confusion colors. The number of points, which the examination may be made to cover, is infinite and is conditioned largely by the time necessary to conduct the survey. Hirschberg's double spectroscope (157) has two colimator arms and two slits, giving two spectra, one placed above the other. The upper spectrum is set at a certain color, and the patient is asked to move the other colimator tube until he finds the color that seems to him to match the test color. Normals, of course, will find a match only with the same wave-length, whereas color-blind patients will pick out more than one region to make a match. The spectro-color-mixer of Helmholtz[3] has found frequent use in scientific determination of the color sense, though its intricacy and expensiveness has limited its application as a test for color-blindness. Abney (2) has a "modified apparatus for the measurement of color and its application to the determination of the color sensations," which is really a combination of a spectro-colorimeter and spectro-color-mixer. L. E. Maire (206, 207) has described a chromoscope consisting of a prism with sides 1¼ inches wide and 4¼ inches long, placed in a box at a suitable angle to receive light through fenestrated cards. The observer at 6 feet from a window holds the chromoscope before him and looks through it after the manner of a stereoscope, bringing the card in view. He will see a spectrum. Just how this spectrum is used as a test for color-blindness, Maire does not say, but among 121 individuals tested with this instrument, he found 3 color-blind cases which were verified by the Holmgren test. Edridge-Green (101, 112) uses the spectrum in much the same way as Hirschberg. He finds first the limits of the red and violet ends and then asks the

[5]Helmholtz, Physiologische Optik, 1st ed., page 303.

patient to mark off the monochromatic bands and describe what he sees as the spectrum is moved along. By this means Edridge-Green is able to fit the patient into his class of defects depending upon whether he is able to distinguish two, three, four, or more colors in the spectrum. Hess goes through much the same procedure in his use of the spectrum as a test for color perception. He further establishes the amount of overestimation and underestimation of blue and yellow by red-green–blind persons by means of sectored disks in photometry.

G. Quantitative Tests

There now remains for consideration those tests which have been devised for the especial purpose of giving a quantitative estimate of the color defect. As has been said, the general trend of opinion among experts in the field of color-blindness is towards the point of view that chromatic sensitivity varies from individual to individual in much the same way as do other physiological and psychological traits. It has been found quite generally that there is a gradual transition in degree of defect from the one extreme of total color-blindness, through normal perception of color, to the other of extreme chromatic sensitivity, and scientists have become interested in studying the quantitative aspect of color vision.

Donders (66) was among the first to take up the problem from this point of view. He states that it is possible to determine quantitatively the ability to distinguish colors on the basis of (1) intensity, (2) saturation, and (3) visual angle. It is on these three factors, with the addition of a fourth, (4) time, suggested by Guttmann, that the quantitative tests for color-blindness rest. Donders suggests that intensity may be used as a test by getting the threshold of chromatic perception of colored light by means of sectored disks. Saturation is tested by using Maxwell disks and getting the threshold for colors in grays. As a test object for visual angle, Donders uses round pieces of colored flower paper, 1, 2, 3, and 5 mm. in diameter, each piece pasted on black velvet, and also pieces cut from materials used for the white, red, and green signal flags. The visual angle required to recognize the color of these test objects may be computed either on the basis of the diameter of the test object at a fixed distance or on the basis of the distance required to recognize the color of a test object of

fixed diameter. For a test with transmitted light he uses his lantern and changes either the size of the opening or the intensity of the light, using a fixed distance between the test light and the patient. Snellen (292) uses Donders' method. Weber (318), Dor (69), and Landolt (193) use the visual angle method in much the same way. Parinaud's chromoptometer (247) is based on the principle of the size of diaphragm necessary for color recognition at a fixed distance. Wollfberg (335) made a study of the size of object necessary for normals to recognize the color of Marx papers at 6 meters. He found that a disk 2 mm. in diameter was necessary to recognize red, and one of 7 mm. to recognize blue. These were mounted on black velvet backgrounds and shown to observers at close range and then moved away until the observer could no longer tell the color. The rating was made in terms of distance according to Snellen's method of rating visual acuity. Frank (79) used 8.7 mm. signal lights with the regulation Nels glasses in green and red. The tests were made out-of-doors at night. The candidate was asked to walk up to the light until he could recognize its color, and then walk back until the color was lost. According to Sauvineau (279), candidates for the railroad service should be able to recognize the color of 2 mm. disks at 5 meters. For transmitted light he uses diaphragms of 1, 2, 3 . . . . 10 mm. diameters and for reflected light he uses little paper disks constructed on the same scheme. The task appears to be one of recognition, but Sauvineau does not say whether this is indicated by naming or identification. He rates the color vision in terms of 9/10, 8/10, 7/10, etc.

Guttmann and Rosmanit both discuss the visual-angle method of getting a quantitative estimate of the color sense, but neither is very explicit in the description of the tests used. They also discuss the use of the tachistoscope in determining the difference between normals and color defectives in the amount of time required to recognize the test color. Guttmann (126) found that normals required about 18σ for the recognition of pigment colors, whereas the anomalous required 760σ. He also used spectral light in connection with the tachistoscope and found the difference even more marked, but gives no figures.

Parinaud's apparatus (248) consists of a cylindrical tube, *A*, fixed to a board, *B*, behind which is a source of light. At the back end

of the tube is a convex lens. Fixed in the opening of the board is an Aubert diaphragm. At a certain distance from the diaphragm is a ground-glass plate. This part of the apparatus is a separate unit. The second part has the same arrangement with another diaphragm with a graduated scale and a lens and ground-glass plate. A screen is attached to the end of the frame for the insertion of colored gelatines. Between the screen and the ground-glass, finally, is a rotating disk with 14 apertures, ranging in diameters from 10 mm. to .5 mm. The determination of the acuity for colors is based (1) on a variation in intensity, namely, the minimum necessary to recognize the color as color (the variation is effected by means of the Aubert diaphragm), and (2) on the visual angle, which is represented by the different sized apertures. The patient is seated at from 3 to 5 meters from the apparatus. For reflected light, patches of colored material are slipped into the frame instead of the ground-glass used in testing with transmitted light. Carter has devised a "color sense measurer" (41), suggested by the *Lichtsinnmesser* of Förster. The instrument consists of an oblong box, 12 inches long and 8 inches wide and 7 inches deep, raised upon an adjustable pillar to a convenient height, so as to be nearly on a level with the patient's eyes. In front of the box is an aperture for the admission of light, which may be furnished by a lamp or an electric light. The aperture is filled by a convex lens which has the light at its focus, so that the rays entering the box are rendered parallel, and this lens is covered by a piece of ground-glass. Immediately within the aperture are two metal screens, governed by milled heads, which, as they approach or recede from each other, diminish or enlarge the interval between them for a range of from 0 to 1500 sq. mm. The magnitude of the interval is read off on the scale on the screw. The handle (*H*) controls a screen with three openings, any one of which may be interposed between the box and the light. One of these openings is clear, another filled with a peacock blue glass, and the other with a red glass. There are two sliding tubes inserted on each side, directed towards the back of the box, for the purpose of looking into its interior. The test objects are placed at the back of the box. It carries a revolving disk on which the objects are so arranged that any one of them can be brought into direct line of sight. Some of the objects are opaque and intended to be looked

at through the tubes, while others are translucent. The examiner brings an opaque object into view and, himself looking through one of the tubes, directs the person tested to look through the other and to turn the milled head slowly until he sees something, when he is asked to describe the object. Normals require about 4 sq. mm. and color defectives require from 16 to 80 sq. mm.

Saturation has been used by Maxwell as a test for color blindness (212). With his color disks he found the threshold of color in grays. Kolbe (187) has followed the same principle in the construction of his *Farbenmesser*. This is of the nature of a truncated cone, covered with a combination of gray and colored pigment papers in such a way that when the cone is rotated about its vertical axis, the surface presents different degrees of saturation from end to end. He has a black screen with a slit aperture which may be moved along in front of the cone. The testee is asked to pick out the point where he can just see color, which is to be identified with wools or other color samples. The cone is divided into 100 units of saturation. He finds that for normals the threshold lies below 10; the color-weak require from 20 to 40 units of color, and the color-blind from 50 to 90. He rates the amount of defect by the equation $Fs = \frac{R-r}{R}$, where R is the threshold of the observer, r the normal threshold, and Fs the degree of defect based on Kolbe's arbitrary scale of saturation.

Ole Bull (30) makes a very careful selection of pure pigment colors and mixes them with different amounts of gray selected on the basis of the shade of gray produced when the two complementaries are mixed. He made little squares, 1 cm., of the same intensity, of different hues and different degrees of saturation. Color-weakness is expressed in terms of the proportion of color to gray needed to make the color just perceptible.

Feré (76) constructed a box containing a frame with a cavity 1 mm. thick between two glass plates, into which liquids could be poured. He used solutions of mytheline blue and fuchsine in alcohol. He made different degrees of saturation until the observer could just perceive the color of the solution. The saturation was rated in terms of the concentration of the solution and could be used to give a very accurate and reproducible estimate of the amount of color needed to be just perceptible, i.e., of the chromatic sensitivity of

the observer. Woefflin (331) has recently made use of a similar scheme in recent studies of color sensitivity.

An apparatus to be used for measuring color sensitivity, which permits the control of brightness, saturation, and visual angle, has been designed and described by Ferree and Rand (77, 78).

Within the last few years, due perhaps to the increasingly complicated problem of traffic regulation, a new impetus has been given to the study of color sensitivity. Ophthalmological, medical, psychological, and other scientific journals contain the reports of many studies, but no new principles upon which to base tests for color-blindness have been developed. For the most part, the tests now used are modifications of the type of tests used in the earlier experiments.

BIBLIOGRAPHY*†‡

1. ABELSDORF, G.	Über die Möglichkeit einer objektiven Nachweise der Farbenblindheit.	*Arch. f. Augenhk.*, 1899, **41**, 155.
2. ABNEY, W. DEW.	Modified apparatus for the measurement of color and its application to the determination of the color sensations.	*Phil. Trans. Roy. Soc.*, 1906, A, **205**, 333-335.
3. ADLER, H.	Die Farbstiftprobe.	*Wien. Klin. Woch.*, 1891, **4**, 389.
4. ALLEN, F.	Colour-blindness.	*Nature*, 1910, **83**, 69-70.
5. ANNOTATIONS.		*Lancet*, 1893, **2**, 44.
6. [ANON.]	Dangerous color blindness.	*Brit. Ophth.*, 1922, **6**.
7. ARMAIGNAC, H.	Quelques réflexions sur la vision des couleurs et de la daltonism.	*Ann. d'ocul.*, 1912, **148**, 161.
8. AUBERT, H.	Beobachtungen.	*Graefes Arch. f. Ophth.*, **3**, Abt. 2, 38.

*The bibliography has been compiled from the *Index Medicus, The Medical Quarterly Index, Psychological Review Index,* the *Catalogues* of the Surgeon General's Library in Washington, D. C., the College of Physicians and Surgeons in Philadelphia, the University of Pennsylvania, The Johns Hopkins University, and cross references.

Not all titles in the Bibliography are directly referred to in the text.

†General textbooks on physiology and psychology of vision have not been included in the bibliography.

‡The above bibliography is essentially complete up to the year 1922. Because later works are easily accessible and readily obtainable, it was not considered necessary to include all references of recent work.

9. Augstein, —.	Die praktische Untersuchung auf Farbenblindheit.	*Aerztl. Sachv. Ztg.*, 1905, **11**, 88-90.
10. B., H. N.	Tests for color vision.	*Nature*, 1910, **83**, 208.
11. Badal, G.	Acuita visiva e senso cromatico nelle scuolle.	*Bull. sci. med. Bologna*, 1904, **75**, 381.
12. Baird, J. W.	The problems of color-blindness.	*Psychol. Bull.*, 1908, **5**, 294.
13. Baldwin, F. N. B.		*Lancet*, 1890, 106.
14. ————.		*Lancet*, 1911, **50**, 902.
15. Beaumont, W. M.	The use of color tests in medical and surgical practice.	*Brit. Med. J.*, 1912, 169.
16. Becker, F.	Untersuchungen über den Farbensinn bei Künstlicher Beleuchtung.	*Graefes Arch. f. Ophth.* 1904, **59**, 424.
17. Bell, J.	Colour blindness.	(Nettleship Memor. 2.) Cambridge: Cambridge Univ. Press, 1926.
18. Bickerton, T. A.		*Lancet*, 1886, **2**, 392.
19. ————.	Color blindness in the mercantile marine.	*Practioner*, N.S., 1903, **17**, 203.
20. Black, N. M.	A new series of semaphore charts for testing the vision of R. R. employees.	*Amer. J. Ophth.*, 1905, **22**, 33-38.
21. Bonner, W. F.	Monocular color vision.	*Amer. J. Ophth.*, 1926, 604.
22. ————.	Visual requirements of auto drivers.	*J. Amer. Med. Asso.*, 1923, **81**, 1628.
23. Bostrom, C. G.	Signals at railway crossings.	*Acta. Ophth.*, 1926, **4**, 65.
24. Bradbrook, H. N., & Roaf, H. E.	An examination of the sensitivity to different regions of the spectrum of individuals with hypochromatic vision.	*Quar. J. Exper. Physiol.*, 1925, **15**, 447.
25. Brailey, W. A.	On tests of vision which should be applied to sailors.	*Brit. Med. J.*, 1883, **2**, 1008.
26. Broca, A.	Sur la vision des signeaux colorés et les épreuves de la perception colorée.	*Ann. d'ocul.*, 1902, **127**, 265-271.
27. Brocchi, A.	Sur le sens de la couleur.	*Rev. scient., 4e S.*, 1898, **9**, 270.
28. Brown, M. E.	Color acuity.	*New Orleans Med. J.*, 1926, **76**.
29. ————.	Color acuity, recent studies with test.	*New Orleans Med. J.*, 1926, **79**, 127-129.
30. Bull, O.	A new method of examining and numerically expressing the color percept.	*Trans. Int. Med. Cong.* 1881, **3**, 49.

31. Burch, G. J.	A confusion-test for color-blindness.	*Ophth., London,* 1912.
32. ————.	Confusion-tests for color-blindness.	*Brit. Med. J.,* 1912, **1,** 836.
33. ————.	A confusion-test for color-blindness.	*Amer. J. Ophth.,* 1922, **5,** 737.
34. Burnett, S. W.	Color sense of 3040 colored school children.	*Arch. Ophth.,* **8.**
35. Butler, T. H.	The futility of the official tests for colour blindness.	*Brit. Med. J.,* 1910, **1,** 315.
36. Buxton, —.	Color tests for R.R. servants.	*Lancet,* 1889, 1252.
37. ————.	A combination test for color vision.	*Lancet,* 1893, **2,** 135.
38. Caley, H.	A discussion of visual tests.	*Brit. Med. J.,* 1899, **2,** 766.
39. Carson, W. E.	Visual requirements of the U. S. Navy.	*J. Amer. Med. Asso.,* 1918, **71,** 263.
40. Carter, R. B.	Color vision.	*Rep. of the Smithsonian Instit.,* 1890, **1,** 687-704.
41. ————.	The quantitative determination of color vision.	*Lancet,* 1894, **1,** 665.
42. Cartright, E. H.	An improved wool stick for testing color vision.	*Brit. Med. J.,* 1896, **1,** 602.
43. Chambers, D. A., Todd, T. W., & Iler, H. D.	Stereoscopic projection and Ridgeway's standards as methods of investigating color blindness.	*Amer. J. Physiol.,* 1926, **75,** 591-599.
44. Chance, B.	Color vision.	*Ophth. Lit.,* 1920, **16,** 231; 1921, **17,** 320.
45. ————.	Some aspects of the status of color vision.	*Amer. J. Ophth.,* 1922, **5,** 274-287.
46. ————.	Dangerous color blindness.	*Brit. J. Ophth.,* 1922.
47. Chase, A. E.	Color blindness in industry.	*J. Amer. Med. Asso.,* 1926, **87,** 1258-1261.
48. Clark, J. H.	The Ishihara test for color blindness.	*Amer. J. Physiol. Opt.,* 1924, **5,** 269-276.
49. Clark, L.	Testing for color blindness.	*Nature,* 1890, **43,** 147.
50. Cohn, M.	Gestikte Buchstaben.	*Centbl. f. prakt. Augenhk.,* 1878.
51. ————.	Der Simultankontrast zur Diagnose der Farbenblindheit.	*Centbl. f. prakt. Augenhk.,* 1878.
52. ————.	Neue Untersuchungen des Farbensinnes.	*Centbl. f. prakt. Augenhk.,* 1881.
53. ————.		*Brit. Med. J.,* 1892, **2,** 626.

54.	———.	Lichtprüfer fur Arbeitsplätze, etc.	*Woch. f. Ther. u. Hygiene d. Auges,* 1899, **3.**
55.	———.	Täfelchen zur Prüfung des Farbensinnes.	Berlin, 1904.
56.	Cohn, M., & Magnus, —.		*Dtsch. Med. Woch.,* **4,** 2.
57.	Colline, G. L.	Color blindness, its relation to other ocular functions and the bearing on public health of tests for color sense acuity.	Washington, D. C., Gov. Print. Office, *Pub. Health Bull.,* No. 2.
58.	Collins, M.	Color blindness, with a comparison of different methods of testing.	New York: Harcourt, Brace, 1926.
59.	Cordiero, F. J. B.	Color perception.	*N. Y. Med. J.,* 1898, **67,** 712.
60.	Cowdrick, M., & Winfield, M.	The adjustment of the Hering color-blindness apparatus.	*Amer. J. Psychol.,* 1919, **30,** 418-422.
61.	Daae, A.	Die Farbenblindheit und ihre Entdeckung.	*Centbl. f. prakt. Augenhk.,* 1878.
62.	Dennet, W. S.	Holmgren and Thomson worsted in a new form.	*Trans. Amer. Ophth. Soc.,* 1886, **4,** Pt. 2.
63.	Despagnet, —.	L'acuité visuelle et la faculté chromatique chez les agents actifs des chemin de fers.	*Rec. d'ophth.* 3e S., 1900, **32,** 460-466.
64.	Digby, K. H.	The Admiralty tests for color blindness.	*Lancet,* 1912, **1,** 1154.
65.	Dieterici, C., & König, A.		*Zsch. f. Psychol.,* 1893, **4,** 241.
66.	Donders, F. C.	Die quantitative Bestimmung des Farbenunterscheidungsvermögen.	*Graefes Arch f. Ophth.,* 1877, **23,** 282.
67.	———.		*Ann. d'ocul.,* 1879.
68.	———.		*Graefes Arch. f. Ophth.,* 1884, **30,** 15.
69.	Dor, H.	Eschelle pour mésurer l'acuité de la vision chromatique.	Paris, 1878.
70.	Dunn, P.	Color test appliance.	*Lancet,* 1904, **1,** 1133.
71.	Engelking, E.	Über die Pupillenreaktion bei angeborener totaler Farbenblindheit.	*Klin. Monatsb. f. Augenhk.,* 1921, **66,** 707.
72.	———.		*Med. Klin.,* 1927, **23,** 110.
73.	Erb, A.	Zur Frage der Augenuntersuchungen des Eisenbahnpersonals.	*Schweiz. Med. Woch.,* 1920, **52.**

74. ERDMANN, P.	Die Anwendung Blauer Gläser bei der Untersuchung mit künstilichem Licht nebst Beschreibung eines einfachen Apparates zur praktischen Prüfung des Farbensinnes.	*Münch. Med. Woch.*, 1905, **52**, 161.
75. EVERBUSCH, O.	Ein Apparat zur praktischen Untersuchung des Farbesinnes beim eisenbahn und marine Personal.	*Graefes Arch. f. Ophth.*, 1900, **50**, 151.
76. FERÉ, C.	Boîtes chromatoscopique pour l'éxploration et l'exercise de la vision des couleurs.	*C. r. Soc. de biol.*, 1897, **4**, 877.
77. FERREE, C. E., & RAND, G.	An apparatus for testing the light and color sense.	*Amer. J. Ophth.*, 1920, **3**, 812-814.
78. ———.	Acuity apparatus with attachments for testing light and color sense and for color mixing.	*Arch. Ophth.*, 1926, **55**, 245-256.
79. FRANK, M.	A comparative study of normal and subnormal color perception in its relation to distant signal lights.	*Ophth. Rec.*, 1904, **13**, 358.
80. FRIDENBERG, P.	Über die Wahrnehmung der Farben.	*N. Y. Med. Monatsschr.*, 1898, **10**.
81. ———.	A test for central color perception.	*Arch. Ophth.*, 1903, **32**, 269.
82. ———.	A signal test for color blindness.	*Med. Rec.*, 1903, **64**, 175.
83. GARTEN, S.	Hering's color mixing apparatus for the study of spectral lights.	*Zsch. Biol.*, 1920, **73**, 89.
84. ———.		*Zsch. f. Augenhk.*, 1922, **47**, 187.
85. GEISSLER, A.	Die Farbenblindheit, ihre Prüfungsmethoden und ihre praktische Bedeutung.	1882.
86. GELB, A.	Farbenpsychologische Untersuchungen.	*Zsch. f. Psychol.*, 1923, **93**, 83.
87. GERTZ, —.	Bemerkungen über das centrale Sehen bei der angeborenen totalen Farbenblindheit.	*Arch. f. Augenhk.*, 1911, **70**, 202.
88. GIBSON, C. R.	An exhibition of some experiments on color blindness.	*Nature*, 1919, **102**, 299.

89.	Gildemeister, M.	Über die Erlernung von Farbenungleichungen.	*Graefes Arch. f. Ophth.*, 1921, **107**, 26.
90.	Goertz, —.	Zur Prüfung auf Farbenblindheit.	*Münch. Med. Woch.*, 1897, **44**, 196.
91.	Göthlin, G. F.	Polarisationsanomaloskopet en apparat for diagnostik undersökning of fargsinnet.	*Svenska Lakarsallskopet Handl.*, 1916.
92.	Edridge-Green, F. W.	Correspondence.	*Lancet*, 1889.
93.	———.	Two new tests for colour blindness.	*Brit. Med. J.*, 1890, **1**, 75.
94.	———.		*Brit. Med. J.*, 1891, **2**, 470.
95.	———.	Remarks on colour blindness and the tests to be adopted for its detection.	*Ophth. Rev.*, 1900, **19**, 271-279.
96.	———.	The evolution of the colour sense.	*Trans. Ophth. Soc. London*, 1901, **21**, 182-199.
97.	———.	Requirements for a test for colour blindness.	*Ophth. Rev.*, 1902, **21**, 31-42.
98.	———.	Les laines de Holmgren comme moyen de déceler le daltonism.	*Ann. d'ocul.*, 1903, **128**, 461.
99.	———.	On the necessity for the use of colour names in a test for colour blindness.	*Trans. Ophth. Soc. London*, 1903, **23**, 210-215.
100.	———.	The tests for colour blindness.	*Trans. Ophth. Soc. London*, 1903, **23**, 206-210.
101.	———.	Colour blindness and colour perception.	London, 1909, 2.
102.	———.	Tests for colour-blindness.	*Nature*, 1910, **84**, 495.
103.	———.	Hunterian lectures.	*Lancet*, 1911, **1**, 285-358.
104.	———.	Lantern test for colour blindness.	*Lancet*, 1911, **1**, 616.
105.	———.	A new test for colour blindness.	*Proc. Physiol. Soc. London*, 1911-1912.
106.	———.	Colour vision and colour blindness.	London: Med. Press, 1912, N.S., **110**, 514-517.
107.	———.	Necessity for the use of colour names in a test for colour blindness.	*Brit. Med. J.*, 1912, **2**, 11-13.
108.	———.	The Admiralty tests for colour blindness.	*Lancet*, 1912, **1**, 1019.
109.	———.	Sight tests of the Board of Trade.	*Lancet*, 1913, **1**, 1557.
110.	———.	Card test for colour blindness.	*Proc. Roy. Soc. Med. London, Sec. Ophth.*, 1920, **13**, 62.

111. ————.		*Brit. J. Ophth.*, 1920, **4**.
112. ————.	The physiology of vision.	London, 1920.
113. ————.	Standard of rejection for colour blindness in seamen.	*Brit. J. Ophth.*, 1922.
114. ————.	New researches in colour vision, Arris and Gale lectures.	*Lancet*, 1922.
115. ————.	Colour vision theories in relation to colour blindness.	*Rep. Brit. Asso. Adv. Sci.*, 1922, 391.
116. ————.	Colour blindness.	*Brit. Med. J.*, 1926, 1060.
117. GRILL, C.	Untersuchungen über simultanen Farbenkontrast bei einem anomaltrichromatischen Sehorgan.	*Upsala Lakaref Fordh.*, 1922, **23**, 367-375.
118. GROETHUSEN, —.	Über die Beziehungen zwischen motorischer und optischer Unterschieds empfindlichkeit des Auges.	*Arch. J. Augenhk.*, 1920, **86**, 20-27; 1920, **87**, 152-158; 1921, **88**, 83-115.
119. GROSSMAN, K.	Colour blindness, with demonstration of new tests.	*Brit. Med. J.*, 1888, **2**, 1041.
120. GROW, E. J.	Remarks on color blindness.	*U. S. Naval Med. Bull.*, 1921, **15**, 717.
121. ————.	Remarks on color blindness.	*Mil. Surg.*, 1921, **49**.
122. GRUNER, —.	Color sense tests with colored light objects.	*Acta. Ophth.*, 1925, **3**, 149-152.
123. GUAITA, L.	Metoda per mesurare la visione cromatica e la sensibilita luminosa.	*Ann. di ott.*, 1895, **24**, 55.
124. GUTTMANN, A.	Untersuchungen an sogenannten Farbenschwachen.	*Ber. d. I. Kong. f. Psychol.*, 1904.
125. ————.	Untersuchungen über Farbenschwäche.	*Zsch. f. Sinnesphysiol.*, 1906, **41**, 45.
126. ————.	Untersuchungen über Farbenschwäche.	*Zsch. f. Sinnesphysiol.*, 1907, **42**, 24-64; 250-270.
127. ————.	Untersuchungen über Farbenschwäche.	*Zsch. f. Sinnesphysiol.*, 1908, **43**, 146-162; 199-223; 255-298.
128. ————.	Anomale Nachbilder.	*Zsch. f. Psychol.*, 1910, **57**, 271-292.
129. ————.	Demonstration der Wichtigsten Methoden zur Untersuchung des Farbensinnes.	*Ber. d. Kong. f. Exper. Psychol.*, 1912.

130.	HAMILTON, W. F., & LAURENS, H.	The sensibility of the fatigued eye to differences in wave-lengths in relation to color blindness.	*Amer. J. Physiol.*, 1923, **45**, 569-584.
131.	HARTRIDGE, H.	Colour blindness and Young's hypothesis.	*Brit. J. Ophth.*, 1920, **4**, 318.
132.	HAYES, S. P.	Color sensations of the partially color-blind.	*Amer. J. Psychol.*, 1911, **22**, 369-407.
133.	———.	Vision, color defect.	*Psychol. Bull.*, 1916, **13**, 131-134.
134.	———.	Color defects, measurement, classification, heredity.	*Psychol. Bull.*, 1926, **23**, 547-573.
135.	HEAD, —.	Tests for color blindness.	*Med. Rec. N. Y.*, 1880, **17**, 496.
136.	HEGENER, C. A.	Zur Diagnostik der Farbenstörungen.	*Schweiz. Med. Woch.*, 1920, **50**, 313.
137.	———.	Zur Methodik der Sehprüfung.	*Arch. f. Augenhk.*, 1921, **88**, 42-57.
138.	HEMBOLT, —.	Beitrag zur Prüfung des Farbensinnes mit Pigmentfarben.	*Arch. f. Augenhk.*, 1913, **75**, 381.
139.	———.	Eine Methode der Farbensinnprüfung mit variablen Pigment-farbengleichungen auf drehbaren Scheiben.	Wiesbaden: J. F. Bergman.
140.	HENSON, V. A. C.	The detection of color blindness.	*J. Phil. & Psychol.*, 1906, **3**.
141.	HERING, E.		*Lotos*, N. F., 1880, **1**.
142.	———.	Über Individuelle Verschiedenheiten des Farbensinnes.	*Lotos*, 1885, **4**.
143.	———.	Zur Diagnostik der Farbenblindheit.	*Graefes. Arch. f. Ophth.*, 1890, **36**.
144.	———.		*Pflüg. Arch. f. d. ges. Physiol.*, **42**, 119.
145.	———.	Zur Diagnostik der Farbenblindheit.	*Arch. f. Ophth.*, 1890, **36**, 217-233.
146.	HERRICK, C. L.	Color vision.	*J. Comp. Neur. & Psychol.*, 1904, **14**, 274-280.
147.	HESS, C.	Das Differentialpupillis-kop.	*Arch. J. Augenhk.*, 1916, **80**, 22-31.
148.	———.	Einfache Apparate zur Untersuchung des Farbensinnes und seiner Störungen.	*Arch. f. Augenhk.*, 1920, **42**, 273.
149.	———.	Die Farbensinnprüfungen des Bahn und Schiffspersonal und die Notwendigkeit ihrer neuen Gestaltung.	*Med. Klin. Berlin*, 1920, **16**, 1279.

150. ————.	Die Rotgrünblindheiten.	*Pflüg. Arch. d. ges. Physiol.*, 1920, **185**, 147.
151. ————.		*Zsch. f. Augenhk.*, 1920, **43**, 28.
152. ————.	Farbenlehre.	*Ergebn. d. Physiol.*, 1922, **20**, 1-107.
153. ————.	Methoden zur Untersuchung des Licht und Farbensinnes sowie des Pupillenspieles.	*Handb. d. biol. Arbeitsmethod.*, Abt. V. Berlin: Urban und Schwarzenberg, 1922.
154. Hilbert, R.	Die Individuellen Verschiedeheiten des Farbensinnes zwichen den Augen eines Beobachters.	*Pflüg. Arch. f. d. ges Physiol.*, **57**, 61.
155. ————.	Ein bequemes Hilfsmittel zur Diagnose der Farbenblindheit.	*Graefes Arch. f. Ophth.*, 1883, **13**, 383.
156. Hirlinger, —.	Prüfungstafeln für Farbensinn.	Stuttgart, 1878.
157. Hirschberg, J.	Spektroskop zur Prüfung des Farbensinnes.	*Arch. f. Anat. u. Physiol.*, Physiol. Abt. 1 & 2, 169.
158. Hoffmann, P.	Die Bedeutung der Farbenblindheit für die Schule.	Leipzig, 1907.
159. Holmgren, —.	Om fargsinnet i vissa del synfaltet.	*Upsala Laharef. Forh.*, 1896, **1**, 485.
160. ————.	Color blindness in its relation to accidents.	Translated for the Smithsonian Instit. Report for 1877.
161. Holth, —.	Meine drei objektiven Proben für zentrales Skotom.	*Klin. Monatssch. f. Augenhk.*, 1921, **66**, 166.
162. Houston, R. A.	A new method of investigation of colour blindness.	*Proc. Roy. Soc. Edin.*, 1921, **42**.
163. ————.	An investigation of the colour vision of 527 students by the Rayleigh test.	*Proc. Roy. Soc. London*, 1922, **A102**, 353.
164. ————.	A new instrument for investigating colour vision.	*Proc. Opt. Conv.*, 1926, 148-155.
165. Iler, H. D., Chambers, D. A., & Todd, T. W.	Stereoscopic projection and Ridgeway's standards as methods of investigating color blindness.	*Amer. J. Physiol.*, 1926, **75**, 591-599.
166. Ishihara, —.	Tests for color blindness.	1921.
167. Ives, H. E.	Simultaneous color measurements by trichromatic and monochromatic analysis.	*J. Opt. Soc. Amer.*, 1923, **7**, 287.

168.	Jeaffreson, —.	The colour circle.	*Lancet,* 1886, **2**, 115.
169.	Jeffries, J.	Color blindness, its dangers and detection.	Houghton, Osgood, 1879.
170.	Jennings, J. E.	Color-vision and color-blindness.	Philadelphia: F. A. Davis, 1896. Pp. 115. (2nd ed.) 1905.
171.	————.	A self-recording device for testing color sense.	*J. Amer. Med. Asso.,* 1914, **63**, 1030.
172.	————.	The present status of color-blindness with demonstration of a new test.	*J. Mo. Med. Asso.,* 1915, **12**, 251-255.
173.	————.	Clinical and office methods of testing color-blindness.	*Amer. J. Physiol. Opt.,* 1920, **1**, 77-93.
174.	————.	Lantern.	*J. Amer. Med. Asso.,* Sec. Ophth., 1920, 235.
175.	————.	Second edition of the self-recording test.	*Amer. J. Ophth.,* 1922, 270.
176.	————.	Test for color perception.	*Amer. J. Ophth.,* 1926, **9**, 221.
177.	Keersmacker, —.	Le Daltonisme.	Brussels, 1884.
178.	————.		*Rev. d'ophth.,* **4**, 1881.
179.	Kennett-Scott, —.	L'essai practique pour la perception des couleurs.	*Ann. d'ocul.,* 1903, **130**, 269.
180.	Kirschmann, —.		*Phil. Stud.,* 1893, **8**, 173-407.
181.	————.	Gläser fur Farbenblinde.	*Dtsch. Opt. Woch.,* 1915, **43**, 607.
182.	Kleinguti, R., & Vogt, A.	Weitere Untersuchungen über die Enstehung der Rotgrünblindheit beim Weibe.	*Arch. f. Rassen & Gesellschbiol.,* 1923, **14**, 130-140.
183.	Klingelhoeffer, W.	Der Farbensinn des Menschen und Seine angeborenen Störungen.	*Naturw. Woch.,* 1921, **20**, 321.
184.	Knies, M.	Das Chromoscope.	*Arch. f. Augenhk.,* 1898, **37**, 225-233.
185.	————.	On a frequent but hitherto unrecognized form of violet blindness and color anomalies in general.	*Arch. Ophth.,* 1900, **29.**
186.	Kolbe, B.	Über die zweckmässigsten Methoden zur Untersuchung des Farbensinnes.	*Centbl. f. prakt. Augenhk.,* 1881, 368.
187.	————.	Beitrag zur quantitative und qualitativen Prüfung des Farbensinnes.	*Arch. f. Augenhk.,* 1883, **13**, 53.
188.	Köllner, H.	Die Störungen des Farbensinnes.	1912.

189. ————.	Fortschritte in der Erkenntniss der Farbensinnstörungen.	*Dtsch. Med. Woch.*, 1924, **8**, 228.
190. ————.	Über Übergänge zwischen normalen Farbensinn und angeborener Rotgrunblindheit.	*Arch. f. Augenhk.*, 1924, **78**, 302.
191. ————.	Die Übergänge zwischen Farbenblindheit und normalen Farbensinn.	*Sitzber. d. Phys. Med. Ges. z. Würzburg*, 1914.
192. König, A., & Dieterici, C.	Die Grundempfindung in normalen und abnormalen Farbensystemen und ihre Intensitätsverteilung im Spektrum.	*Zsch. f. Psychol.*, 1892, **4**, 241.
193. Landolt, E.	Procède pour determination de la perception des couleurs.	*Ann. d'ocul.*, 1875-1877.
194. Laurens, H., & Hamilton, W. F.	The sensibility of the fatigued eye to differences in wave-lengths in relation to color blindness.	*Amer. J. Physiol.*, 1923, **45**, 569-584.
195. Lediard, H. A.	Color test for R.R. purposes.	*Lancet*, 1889, 1056.
196. Lobsien, M.	Über die Farbenkenntnisse bei Schulkindern.	*Zsch. f. Psychol.*, 1904, **34**, 29.
197. Lodge, O. J.	Testing for color blindness.	*Nature*, 1890, **42**, 100.
198. Lovibond, F. E.	Color test by means of Lovibond's tintometer.	*Proc. Opt. Conv.*, 1926, 211.
199. Lovibond, J. W.	Measurement of light and color sensations.	London.
200. Lummer, O.	Observations on vision, with an hypothesis on the cause of color blindness.	Translated for Smithsonian Instit. from *Dtsch. Physiol.*
201. Mackay, G.	On the quantitative estimation of the colour sense.	*Brit. Med. J.*, 1892, **2**, 626.
202. Magnani, —.	Apperchio per la mescolanza dei colori e loro graduazione automatico.	*Ann. di ott.*, 1901, **30**, 61.
203. Magnus, —.	Beiträge zur Kenntniss der physiologischen Farbenblindheit.	*Graefes Arch. f. Ophth.*, 1879, **24**.
204. ————.	Examination for color blindness.	*Boston Med., & Surg. J.*, 1880, 102.
205. Magnus, —., & Cohn, M.		*Dtsch. Med. Woch.*, **4**, 2.
206. Maire, L. E.	A new instrument designed for a ready test of color perception.	*Trans. Mich. Med. Soc.*, 1896, **20**, 182.

207.	————.	The chromoscope.	*Phys. & Surg.*, 1896, **18.**
208.	Marshall, C. D.	Color vision and tests for color blindness.	*Brit. Med. J.*, 1914, 1093.
209.	Matthai, —.	New color test.	*Klin. Monatsb. f. Augenhk.*, 1926, **76**, 143.
210.	Mauthner, L.	Prüfung des Farbensinnes.	*Vorträge aus dem Gesamtgebiet der Augenhk.*, 1879, Heft 4.
211.	————.	Über das Wesen und die Bestimmung der Farben.	*Med. u. Chir. Centbl., Wien.*, 1881, **16.**
212.	Maxwell, J. Clerk.	Experiments on colour and colour blindness	*Trans. Roy. Soc. Edin.*, 1855, **21.**
213.	————.		*Phil. Mag.*, 1857, **4**, 40.
214.	————.		*Nature*, 1871, **4**, 13.
215.	Meisling, A. A.	Color sense and color sense tests.	Abst. of paper read at VI. Scand. Ophth. Cong., 1926, A.J.O., 1926.
216.	Mentz, P.	Untersuchungen zur Psychophysik der Farbenempfindungen am Spektrum.	*Phil. Stud.*, 1898, **13**, 481.
217.	Meyer, H.	Über Kontrast und Komplimentarfarben.	*Pogg. Ann.*, 1855, **95**, 170.
218.	Meyers, C. S.	Some observations on the development of the color sense.	*Brit. J. Psychol.*, 1908, **2**, 353.
219.	Milscher, K.	Beiträge zur Farbenlehre.	*Zsch. f. Sinnesphysiol.*, 1925, **57.**
220.	Mitchel, C. J. C.	The vision of railwaymen.	*Brit. J. Ophth.*, 1922, 319.
221.	Müller, E.	Weitere Bemerkungen über Farbensinnprüfung bei der deutschen Handelsmarine.	*Aerzt. Sachv. Ztg.*, 1907, **13.**
222.	Müller, G. E.	Zur Psychophysik der Gesichtsempfindungen.	*Zsch. f. Psychol.*, 1896, **10**, 1-82; 321-412.
223.	————.	Zur Psychophysik der Gesichtsempfindungen.	*Zsch. f. Psychol.*, 1897, **14**, 1-76; 161-196.
224.	————.	Die Theorie der Gegenfarben und der Farbenblindheit.	*Ber. d. I. Kong. f. Psychol.*, 1904.
225.	Munsell, A. E. O.	Color sensitivity tests.	*Amer. J. Ophth.*, 1926, **9**, 844.
226.	————.	Tests for color vision.	*Lancet*, 1926, Oct., 817.
227.	Nagel, W.	Beiträge zur Symptomatologie, Diagnostik, und Statistik der Farbenblindheit.	*Arch. f. Augenhk.*, 1898, **37.**
228.	————.	Was ergeben die neueren physiologischen Erfahrungen, etc.	*Aerzt. Sachv. Ztg.*, 1904, **10**, 176.

229. ————.	Observations on the color sense of a child.	*J. Comp. Neur. & Psychol.*, 1906, **16**, 217.
230. ————.	Versuche mit Eisenbahn Signalen.	*Zsch. f. Sinnesphysiol.*, 1907, **42**.
231. ————.	Tafeln zur Untersuchung des Farbenunterscheidungsvermögen.	Wiesbaden, 1906. (4th ed.)
232. ————.	Die Farbensinnprüfung bei der Deutschen Handelsmarine.	*Aerzt. Sachv. Ztg.*, 1907, **13**, 229.
233. ————.	Zwei Apparate fur die augenärztliche Funktionsprüfung.	*Zsch. f. Augenhk.*, 1907, Heft 3, 216-222.
234. ————.	Einführung in die Kenntniss der Farbensinnstörungen und ihre Diagnose.	Wiesbaden, 1908.
235. v. NARDROFF, E. R.	A new apparatus for the study of color phenomena.	*Phys. Rev.*, 1896, **3**, 306.
236. v. NEUPERT, J.	Physical examination with special reference to defects of color perceptions.	*Railway Surgeon*, 1912, **18**, 227-258.
237. NEIFELD, M. R.	The color sensation theory of Dr. Schantz.	*Amer. J. Physiol. Opt.*, 1924.
238. NELSON, W. G.	A device for testing the color sense.	*J. Amer. Med. Asso.*, 1925, **85**, 1220.
239. NYDEGGER, J. A.	The importance of adopting a more accurate method of examination for color blindness.	*Med. Rec. N. Y.*, 1913, **83**, 1068-1071.
240. OGUCHI, C.	Zur Kenntniss des Farbensinnes und seiner Störungen.	*Arch. f. Augenhk.*, 1914, **77**, 205-221.
241. OLIVER, C. A.	Color perception, normal and abnormal.	Reference Handbk. of the Med. Sciences, 1901, **3**.
242. ————.	A modification of the Abney Pellet test.	*Ann. Ophth.*, 1902.
243. ————.	Some of the inefficiencies of the methods, etc.	*Ann. Ophth.*, 1896, **5**, No. .4
244. ————.	Description of a series of tests for the detection and determination of sub-normal color perception.	*Trans. Amer. Ophth. Soc.*, 1888, **1**.
245. ÖLOFF, —.	Die Untersuchung Angeborener Farbenblindheit.	*Dtsch. Mil. Arzt. Ztsch.*, 1914, **43**, 41-55.
246. OLOFF, —., & STARGARDT, —.	Über die Bedeutung und die Methodik der Farbensinnuntersuchung.	*Ztsch. f. Augenhk.*, 1912, **28**, 1-24.
247. PARINAUD, H.	The sensitivity of the eye to colors of the spectrum.	*J. Ophth. Otol. Laryng.*, 1896, **8**, 51.

248.	———.	Determination numerique de l'acuité visuelle pour les couleurs et la lumière chromoptomètre.	*Ann. d'ocul.*, 1881, **85**, 113.
249.	Peddie, W.	A color vision spectrometer.	*Proc. Opt. Conv.*, 1926, 155.
250.	Pflüger, E.	Methoden zur Untersuchung des Farbensinnes.	*Correspbl. f. Schweiz. Aerzte.*, 1879, **9**.
251.	———.	Methode zur Prüfung des Farbensinnes mit Tafeln.	1882.
252.	Podesta, —.	Wandtafeln zur Prüfung des Farbensinnes und Erkennung der Farbenstörungen.	Hamburg: D. Frederickson, 1916.
253.	———.	Sehleistung und Farbensinn bei der Münsterschen Studentenschaft.	*Münch. med. Woch.*, 1924, **71**.
254.	———.	The problem of color blindness.	*Brit. Med. J.*, 1915, **1**, 79.
255.	Pryn, W. W.	Tests for color vision.	*Brit. Med. J.*, 1910, **2**, 437-439.
256.	Raehmann, E.	Einige neue Resultate bei der Untersuchung relativer Farbenblinder.	*Pflüg. Arch. f. d. ges. Physiol.*, 1900, **80**, 583.
257.	Rand, G., & Ferree, C. E.	An apparatus for testing the light and color sense.	*Amer. J. Ophth.*, 1920, **3**, 812-814.
258.	———.	Acuity apparatus with attachments for testing light and color sense and for color mixing.	*Arch. Ophth.*, 1926, **55**, 245-256.
259.	Rayleigh, —.	Some experiments in color.	*Nature*, 1871, **3**, 234.
260.	———.	Some experiments in color.	*Nature*, 1881, **25**, 64.
261.	———.	Report of the committee on color vision.	*J. Amer. Med. Asso.*, 1892.
262.	———.	Report of the committee on standards and methods of examining color vision.	*J. Amer. Med. Asso.*, 1914, Sec. Ophth.
263.	v. Reuss, A.	Wolltäfelchen zur Untersuchung auf Farbenblindheit.	*Wien med. Presse.*, 1886, **27**, 75.
264.	———.	Über Farbenblindheit.	*Wien Klin.*, 1879.
265.	Richardson-Robinson, F.	A case of color blindness to yellow and blue.	*Amer. J. Psychol.*, 1923, **34**, 157-184.
266.	Roaf, H. E., & Bradbrooke, H. N.	An examination of the sensitivity to different regions of the spectrum of individuals with hypochromatic vision.	*Quar. J. Exper. Physiol.*, 1925, **15**, 447-450.

267. Roaf, H. E.	A simple device whereby some color blind persons can recognize color differences.	*J. Physiol.*, 1924, **59**, 29-32.
268. Roberts, C.	The detection of color blindness and imperfect eyesight.	London, 1884.
269. ————.	Color blindnes as a facial characteristic.	*Lancet*, 1882, **1**, 124.
270. Robinson, F. R.	A case of color blindness to yellow and blue.	*Amer. J. Psychol.*, 1923, **34**, 157-184.
271. Rochat, G. F.	Apparat zur Diagnose der Farbensinnanomalien.	*Niederl. Ophth. Ges.*, 1914, **14**, 6-10.
272. Rose, E.		*Pogg. Ann.*, 1865, 126.
273. ————.		*Berliner Klin. Woch.*, 1865, **31**.
274. ————.		*Virchow's Arch. f. path. Anat.*, **28**.
275. Rosmanit, J.	Zur Farbensinnprüfung im Eisenbahn und Marine Dienst.	*Wien. Klin. Woch.*, 1908, **21**, 430.
276. ————.	Anleitung zur Feststellung der Farbentüchtigkeit.	Wien: Deuticke, 1914. Pp. v+195.
277. Sachs, M.	Über den Einfluss farbiger Lichter auf die Weite der Pupille.	*Pflüg. Arch. f. d. ges. Physiol.*, **52**, 79-86.
278. ————.	Eine Methode der objektiven Prüfung der Farbenblindheit.	*Graefes Arch. f. Ophth.*, 1893, **39**, 108.
279. Sauvineau, C.	Échelle centesimale pour la mensuration du sens chromatique.	*Ann. d'ocul.*, 1908, **140**, 13.
280. Schaaff, E.	Practical examination of color perception.	*Arch. d'ophth.*, 1927, **44**, 556-565.
281. Schenkle, —.	Die Behilfe zur Diagnose der Rotgrünblindheit.	*Prag. med. Woch.*, 1881, **6**, 181.
282. Schiotz, I.	Color-blind females: the inheritance of color-blindness in man.	*Brit. J. Ophth.*, 1920, **4**, 345; 359; 393; 403; 503.
283. ————-.	Rotgrünblindheit als Erbeigenschaft.	*Klin. Monatsbl. f. Augenhk.*, 1922, **68**, 498-526.
284. Schumann, F.	Ein ungewöhnlicher Fall von Farbenblindheit.	*Ber. d. I. Kongress f. Psychol.*, 1904.
285. Schwechten, E.	Farbentüchtigkeit im Eisenbahndienst.	*Aerztl. Sachv. Ztg.*
286. Scripture, E. W.	The color sense tester.	*Yale Univ. Stud. in Psychol.*, 1900, **8**, 1-20.
287. Seebeck, A.	Über die bei manchen Personen vorkommenden Mangel an Farbensinn.	*Pogg. Ann.*, 1837, **42**, 177.

288.	Sewall, H.	A simple method of testing for color blindness.	*Med. News, Phila.*, 1886, **49.**
289.	Shaxby, J. H.	Double collimator spectrometer for use in physiological optics.	*Quar. J. Exper. Physiol.*, 1927, **16**, 373-378.
290.	Sinclair, W.	Color vision and accidents.	*Lancet*, 1895, 3735.
291.	Snellen, E. C.	The testing of color sense and vision.	*Brit. Med. J.*, 1883, **2**, 1009.
292.	Snellen, H.	La determination quantitative du sens chromatique.	*Nederl. Tijdschr. v. Geneesk.*, 1897, **33**, 688-695.
293.	Snyder, W. H.	The color sense and color blindness.	*Toledo Med. & Surg. Report*, 1896, **9**, 23-27.
294.	Stargardt, —., & Oloff, —.	Über die Bedeutung und die Methodik der Farbensinnuntersuchung.	*Zsch. f. Augenhk.*, 1912, **28**, 1-24.
295.	Stilling, J.	Über das Sehen der Farbenblinden mit Tafeln.	1883.
296.	————.	Pseudoisochromatische Tafeln.	Leipzig.
297.	————.	Über Entstehung und Wesen der Anomalien des Farbensinnes.	*Zsch. f. Sinnes-physiol.*, 1910, **44**, 321-427.
298.	————.	Einige Bemerkungen über die Farbenprüfung.	*Centbl. f. Prakt. Augenhk.*, 1913, **37**, 65-69.
299.	Stratton, G. M.	Railway accidents and the color sense.	*Pop. Sci. Mo.*, 1903, **72**, 244.
300.	Strong, C., & Winfield, M.	The Hering color-blindness apparatus and the normal Rayleigh equation.	*Amer. J. Psychol.*, 1921, **32**, 425-428.
301.	Subina, A.	Über Farbensinn bei Kindern nebst Bemerkungen über die untersuchungs Methoden.	*Russ. Ophth. J.*, 1923, **2.**
302.	Tangemann, C. W.	The value of examinations for sight, color sense and hearing.	*Railway Surg.*, 1896, **30**, 197.
303.	Taylor, G. H.	Clinical examination for color blindness by the lantern test.	*Ophth.*, Seattle, 1904, **10.**
304.	————.	Color testing and the psychology of color.	*Amer. J. Psychol.*, 1924, **35**, 185-190.
305.	————.	Color testing.	*Ophth.*, 1915, **12**, 91-100.
306.	de Theirry, M.	Sur un nouvel appareil dit monochromatoscope.	*Comptes Rend.*, **118**, 636.
307.	Thomson, W.		*Trans. Amer. Ophth. Soc.*, 1880.
308.	————.		*Med. News, Phila.*, 1882.

309.	THOMSON, W. & WEILAND, C.	Normal color perception.	*System,* Diseases of the Eye, Phila., 1897, **1**, 581.
310.	TODD, T. W., ILER, H. D., & CHAMBERS, D. A.	Stereoscopic projection and Ridgeway's standards as methods of investigating color blindness.	*Amer. J. Physiol.,* 1926, **75**, 591-599.
311.	TRIBLE, G. B.	A report on the Jennings test for color-blindness.	*U. S. Naval Bull.,* 1917, **11**, 334.
312.	VELTER, E.	Complete color-blindness with anomalies of the macula.	*Amer. J. Ophth.,* 1925, **6**, 935.
313.	VERREY, A.	Nagel's anomaloscope.	*Brit. Med. J.,* 1926, **2**, 1105-1106.
314.	VIERLING, F.	Der Farbengleichungsapparat von Nagel.	*Arch. f. Augenhk.,* 1914, **77**, 242.
315.	———.	Die Stiftprobe und die Verwechselungsfarben der Farbenuntüchtigen.	*Centbl. f. Prakt. Augenhk.,* 1920.
316.	VOGT, A.	Cecité complète pour les couleurs avec presentation de trois cas.	*Rev. Gen. d'ophth.,* 1922, **36**.
317.	VOGT, A., & KLEINGUTI, R.	Weitere Untersuchungen über die Enstehung der Rotgrünblindheit beim Weibe.	*Arch. f. Rassen. & Gesellschbiol.,* 1923, **14**, 130-140.
318.	WEBER, A.	Apparat zur numerischen Prüfung des Farbensinnes.	*Klin. Monatsbl. f. Augenhk.,* 1878, **16**.
319.	WEILAND, C.	How Javal's keratometer may be easily changed into a chromatometer.	*Arch. Ophth.,* 1895, **24**.
320.	WEILAND, C., & THOMSON, W.	Normal color perception.	*System,* Diseases of the Eye, Phila., 1897, **1**, 581.
321.	WILLIAM, C. H.	The best tests for defective color vision.	*Med. Rec.,* 1895, **8**.
322.	———.	Color testing.	*Boston Med. & Surg. J.* 1896.
323.	———.	Uniform test for vision.	*Amer. Ophth. Soc.,* 1899.
324.	———.	The need of a supplementary lantern test.	*Boston Med. & Surg. J.,* 1903.
325.	WILLMS, E.	Systematische Farbensinnprüfung mit dem. v. Hesschen Kreiselversuch und am Perimeter mit peripheriegleichen invariablen Perimeter Objekten.	(Dissertation Freiburg.) Reviewed in *Klin. Monatsbl. f. Augenhk.,* 1927, **78**, 558.
326.	WILSON, G.	Researches on color blindness.	1855.
327.	WILSON-BARKER, D.	Tests for color vision.	*Nature,* 1910, **84**.

328.	Winfield, M., & Cowdrick, M.	The adjustment of the Hering color blindness apparatus.	*Amer. J. Psychol.*, 1919, **30**, 418-422.
329.	Winfield, M., & Strong, C.	The Hering color blindness apparatus and the normal Rayleigh equation.	*Amer. J. Psychol.*, 1921, **32**, 425-428.
330.	Woefflin, E.	Über zwei Untersuchungsmethoden von Farbenanomalien.	*Arch. f. Augenhk.*, 1923, **92**.
331.	———.	Die Farbensinnprüfung.	*Schweiz. Ophth. Ges.*, 1923.
332.	———.	Über eine neue Methode zum Nachweis der Farbenüberempfindlichkeit.	*Dtsch. Ophth. Ges.*, 1924.
333.	———.	Tables with proximal colors for testing red and green visual acuity.	Leipzig: Thieme, 1926. *Amer. J. Ophth.*, 1926, **9**, 914.
334.	Woinow, —.		*Graefes Arch. f. Ophth.*, 1871, **17**.
335.	Wollfberg, L.	Eine einfache Methode die quantitative Farbensinnprüfung diagnostisch zu Verwerthen.	*Klin. Monatsbl. f. Augenhk.*, 1886, **24**.

The Johns Hopkins University
Baltimore, Maryland

TEST DE DALTONISME

(Résumé)

Le but de l'article est de tracer le développement des tests de daltonisme employés pendant la dernière centaine d'années, et les méthodes et les étalons du diagnostic.

Les anomalies de la vision des couleurs présentent deux aspects: (1) théoriques; et (2) pratiques; lesquels ont formé le développement des méthodes de tester le daltonisme et la déficience dans la vision des couleurs. Au point de vue théorique il est important de constater exactement ce que voit le daltonien. Il faut savior sa sensibilité à la couleur: les limites de son spectre visible; les confusions qu'il fait; à vrai dire, toutes ses réactions anomales aux stimuli connus. Seulement de cette façon il est possible de classifier et de différencier la fonction visuelle, ainsi trouvant des données pour la construction de théories visuelles et des concepts de la nature de la vision des couleurs. L'usage des lumières rouge et verte comme signaux dans le chemin de fer et les navires et le danger à la sûreté publique à cause de la possible confusion de ces deux couleurs nous montrent l'aspect pratique du daltonisme. Ce qui est surtout nécessaire dans un test pratique de daltonisme, c'est un moyen infaillible de connaître ceux qui sont incapables de rendre un bon service; il faut que le test soit diagnostique selon les idées de sûreté acceptées partout.

Pour faciliter la discussion, on a groupé les tests sous les titres suivants:

1) Tests de Confusion
   a) Tests de Classification
   b) Tests Pseudoisochromatiques
   c) Tests de Lanterne
2) Tests de Contraste
   a) Contraste de "Flor"
   b) Contraste de Miroir
   c) Ombres Colorées
3) Tests de Réaction pupillaire
4) Tests d'Equation Rayliegh
5) Tests employant des appareils spéciaux désignés pour satisfaire l'aspect pratique des tests
6) Tests de Spectre
7) Tests quantitatifs
   a) Intensité
   b) Saturation
   c) Angle visuel
   d) Temps de stimulation
   e) Temps de réaction

On appuie sur le développement des méthodes et de leur application. Une bibliographie d'environ 300 titres nous offre une compilation complète de la littérature du daltonisme jusqu'à 1920.

HAUPT

ÜBER FARBENPROBEN BEI FARBENBLINDHEIT

(Referat)

Der Zweck dieses Artikels ist die Entwicklung der Farbenproben für Farbenblinde, die man in den letzten hundert Jahren angewandt, hat zu untersuchen, sowie auch die Methode und den Masstab der Diagnose.

Die Anomalien der Farbenwahrnehumng sind (1) theoretisch und (2) praktisch; dem gewäss haben sich die Methoden der Farbenprobe entwickelt. Vom theoretischen Gesichtspunkt aus ist es von Wichtigkeit zu erfahren was der Farbenblinde sieht. Man muss seine Farbenempfindlichkeit kennen sowie die Grenzlinie seines sichtbaren Spektrums; seine Verwechslungen kurz alle anormalen Reaktionen gegen bekannte Reize. Nur auf diese Weise kann man Sehfunktionen unterscheiden und klassifizieren, und zugleich Material zur Entwicklung von Theorien und Begriffen über den Charakter des Farbensinnes gewinnen. Die praktische Seite der Farbenblindheit entsteht durch die Anwendung roter und grüner Signallichter beim Eisenbahn- und Schiffsdienst, und die Gefahr für die öffentliche Sicherheit welche durch die mögliche Verwechslung der beiden Farben entstehen kann. Das Haupterfordernis für eine praktische Farbenprobe ist die Fähigkeit die Dienstunfähigen zu erkennen und auszuscheiden; die Probe muss diagnostisch dem bestimmten Masstab der öffentlichen Sicherheit angepasst werden.

Zum Zweck der Erörterung hat man die verschiedenen Proben unter die folgenden Rubriken gruppirt:

1) Confusions Proben
   a) Klassifizierungs Proben
   b) Pseudoisochromatische Proben
   c) Laternen Proben

2) Kontrast Proben
   a) Flor-Kontraste
   b) Spiegel Kontraste
   c) Farbige Schatten
3) Pupillen Reaktionsproben
4) Rayleigh Gleichung Tests
5) Proben mit besondern Vorrichtungen um den Auforderungen der praktischen Prüfungen zu genügen
6) Proben des Farbenbildes
7) Quantitative Proben
   a) Intensität
   b) Sättigung
   c) Gesichtswinkel
   d) Dauer der Reizung
   e) Dauer der Reaktion

Man betont die Wichtigkeit der Entwicklung der Methoden und ihrer Anwendung. Eine Bibliographie die etwa 300 Title enthält bietet eine vollständige Sammlung der Abhandlungen über Farbenlindheit bis zum Jahre 1920.

HAUPT

REVERSAL OF GALVANOTROPISM IN ECHINODERMS*

From the Laboratory of General Physiology, Harvard University, and the Mount Desert Island Biological Laboratory

W. J. Crozier

I

Reversal of galvanotropic orientation is well known in certain protozoans, either through alteration of the medium (1, 2, 3), or, in *Volvox,* and *Euglena,* according to the state of light adaptation (22, 3, 16). Among metazoans also this type of effect is known to occur "spontaneously" in certain instances (12), or may appear as a function of increased current density (21) or as dependent on the duration of the application of the current (4, 19). The widespread occurrence of galvanotropic phenomena may make it feasible to employ these reversals for precise analysis of behavior changes; the possibility that galvanotropic excitation may be indirectly expressed in quantitative terms (9) adds to the attractiveness of such an attempt. Little has been offered to account for reversals of orientation, aside from the strange suggestion that reversal of phototropic reactions is to be explained in terms of reversible photochemical reactions (13, 14, 15), for which it is difficult to imagine any rational basis; or that reversal of orientation (of Protista) under the influence of reagents may be formulated in terms of effects upon "permeability" (15, 16). The definiteness of the galvanotropic reversals, which may be induced in metazoans as well as in protozoans, demands some more precise treatment. The cathodal galvanotropism of *Lineus* (nemertean) and the anodal galvanotropism of *Echiurus* (gephyrean) are reversed by strychnine, under conditions such that "reversal of inhibition" in the use of antagonistic muscles is also shown (7, 8). Nicotine does not give this effect. Fries (10) has been able to confirm these findings, with planarians, and in the earthworm, in which neither nicotine nor atropine induce galvanotropic reversal; while with *Asellus,* in which reversal of

*Received in the Editorial Office, March 12, 1929.

inhibition is not obtained under strychnine, no reversal of the galvanotropism is shown. Subsequent experiments have shown these effects to occur with great clearness in marine planarians (*Leptoplana*), reversal of galvanotropism being specifically associated with reversal of reaction to touch under the influence of strychnine. In nudibranchs also (*Dendronotus*), although reversal under strychnine is not so diagrammatically clear, neither atropine, pilocarpine, picrotoxin, caffeine, nor veratrin induces detectable change in galvanotropism. In *Dendronotus* the general excitability of the organism may be very greatly increased by caffeine without reversing the orientation in constant current. This point is important in relation to the interpretation of reversals of orientation which occur in some forms when the galvanotropic excitation is much increased. *Leptoplana* shows convulsive twitchings under the influence of camphor, but cathodic orientation continues with all densities of current between 0.2 and 5.0 m.a. per sq.cm. When a normal *Leptoplana* is stimulated both by current and by high intensities of light (9), or mechanically, it swims by rhythmic movements of the laternal margins and undulations of the whole body, but remains cathodic.

Thus reversal of galvanotropism in metazoans may clearly be related to other phenomena in which "reversal of inhibition" is involved. It gives a rather clear instance of reversal of behavior which is under experimental control; and accordingly becomes of interest to establish its essential conditions.

II

The responses of the sea urchin *Strongylocentrotus drobachiensis*, when compared with those of sea stars, serpent stars, and holothurians subesquently considered, permit one to separate the effects of the constant current upon spines, tube feet, and pedicellariae from that upon movements controlled by contractions of body musculature. The spines slowly bend toward the anode, with beautiful precision, and follow reversals of the current with the decision of the needle of an ammeter. The beginning of response is preceded by a perceptible latent period, which will be made the basis of subsequent experiments. The speed of initiation of the response increases when the current density is increased from 2 to 20 m.a. per sq.cm. Tube feet extend only toward the anode; those on the cathodal side are released from attachment, shorten, and bend toward the anode (as do also the stalks of the pedicellariae). With higher current den-

sities, the anodal extension of the tube feet results in a tilting of the body toward the anode. Carlgren (5) observed cathodal galvanotropism in older larvae of several echinoderms.

The orientation of the spines is obviously due to the anodal contraction of their basal muscles. This is not reversed by any of various treatments with strychnine, although the tube feet then fail to become attached to a substratum (as also in sea stars), and the threshold current density is raised. Injection of as much as 1 c.c. of a saturated solution of strychnine sulphate in sea water (through the peristome) is without effect (until the animal dies); so long as the spines still move when a spot on the surface of the test is stimulated mechanically they bend convergently toward the site of irritation, and they still show anodal bending. Other alkaloids were likewise without effect.

These movements may be contrasted with those of the arms of sea stars, which show definite neuromuscular effects of strychnine (17, 18), since even small pieces of the test of the sea urchin, bearing few or many spines, show in the constant current a behavior of the spines, tube feet, and pedicellariae which is identical with that in the intact animal. The galvanotropic orientation is abolished by chloretone at about the moment when reaction to touch disappears.

The holothurian *Cucumaria frondosa* presents a different situation. With lengthwise current, the tube feet bend to the anode, as before, and that end of the body which is toward the cathode shows circular muscle contraction with consequent extension; when the current is transverse, the two ends (particulary the oral) bend to the anode. This behavior is not reversed by caffeine or by atropine. In strychnine solutions the threshold is markedly raised, and the general state of contraction of the animal makes the responses too indefinite to be used.

The effects are clearer in *Leptosynapta,* but it is necessary to employ fairly high current densities—2 to 4 m.a. per sq.cm. A transverse current leads to U-contraction toward the cathode. Longitudinally, the oral end and tentacles expand when toward the cathode; this is reversed with change of current; the cloacal end contracts markedly when toward the cathode. It was not possible to control the direction of peristaltic body waves, which normally course orally.

After the injection of ½ c.c. saturated solution of strychnine sulphate it is found that within half an hour the mouth is relaxed, wide

open, and that the tentacles no longer retract when they or the oral region are even vigorously stimulated, as by pinching or prodding. It is difficult to obtain such clear-cut effects by immersion of the animal in sea water containing strychnine, and the results were never satisfactory. Injected animals subjected to a transverse current show bending of the oral end to the anode; the curvature at the cloacal end is not so clear, and it commonly does not bend at all. This definite reversal of the galvanotropic orientation is seen also in the behavior of the tentacles, which now extend toward the anode. Both types of curvature repeatedly and uniformly follow reversals of the current. If the current be sent longitudinally, the animal extends when the oral end is toward the cathode, and the tentacles are extruded. With current in reverse direction the tentacles are retracted and the body shortens and thickens (sometimes after preliminary anodal bendings of the tentacles). The shortening is succeeded after several minutes by extension of the oral end (tentacles still retracted) and by its curvature toward the anode.

The conspicuous reversal of the behavior of the tentacular region in reaction to local stimulation, and of the galvanotropic orientation, contains several elements of interest. Normally, touching a tentacle (not too gently) results in the partial collapse of the tentacles, the inversion of the tentacular crown and the retraction of the oral disc [as in *Holothuria,* (6); *Synaptula,* (20)]. Similar treatment of the strychninized individual still leads to partial collapse of the tentacles, but the introversion of the oral disc is prevented by the sphincter-wise constriction of the immediately post-oral region of the column. Just behind this constriction there is pronounced contraction of the longitudinal muscles, producing swelling.

Reversal of reaction and other typical effects under the influence of strychnine have been observed in other echinoderms, including holothurians (11) and sea stars (17, 18). The fact that galvanotropic movement is also reversed permits the extension to these forms of the contention earlier advanced, that the drug acts in a specific way upon structures distinct from the nerve cell bodies presumably affected by the application of constant electric current, and which we may therefore provisionally term "synapses."

III

Reversal of galvanotropism is shown by other echinoderms under the influence of strychnine; but, so far as the present experiments

have gone, the alkaloid must be injected to produce this result. Immersion in very dilute solutions of the drug suffice to produce certain well-marked effects, described by Moore (*loc.cit.*), and which must be accepted as aspects of "reversal of inhibition." *Asterias vulgaris* and *Henricia sangulioenta* behave essentially as Moore has stated for *A. forbesi* when treated with strychnine or with nicotine. With the former alkaloid, dorsal flexure of the rays is pronounced, with the latter, ventral flexure. The effect of pilocarpine is similar to that of strychnine, except that the tube feet continue longer to attach to the substratum. Moore (18) found that strychninization of a nicotinized starfish resulted in dorsal flexure of the rays, but that nicotinization of one first strychninized failed to induce the ventral flexure. Nicotine antagonizes and can overcome the strychnine-like effect of pilocarpine, however, although pilocarpine cannot overcome (at least not readily) the posture induced by nicotine. This fact may complicate the interpretation of the neuromuscular effects of these alkaloids (18). The reversals of reaction produced by strychnine seem, however, to be fairly specific. They are produced with great clearness when a small volume of a concentrated solution is injected into the body cavity.

Galvanotropic reactions were obtainable only with rather high current densities, approximately 20 m.a. per sq.cm. (with P.D. of 1 volt) in the case of animals for some time in the laboratory. Smaller specimens, recently collected, react diagrammatically with much lower densities (2 to 5 m.a.). The tube feet are extended only to the anode. Rays on the cathode side are slightly lifted from the substratum. Anodal rays may be somewhat elevated at the tips, which may temporarily turn dorsally toward the cathode. The typical position is with the cathodal rays well extended. If the current density be made quite high (smaller individuals are more sensitive), the rays are all flexed sharply oral-ward. Rays transverse to the lines of current flow twist slightly so as to turn the oral faces somewhat toward the anode. Lateral bending movements of the rays appear after a fairly definite latent period. The rays ultimately swing to the cathode side, although one or two may for a time be pointed anodally. Owing to the manner of extension of the tube feet, creeping is possible only toward the anode; as soon as a tube foot, during progression of the animal, begins to point in the direction of the cathode, its sucker is released. When the current is reversed, corresponding alterations in the disposition of tube feet and

rays follow after a latent interval. Rays pointing toward the cathode are definitely elongated, as compared with the "resting" state, and they shorten quite perceptibly when the current is reversed. When the starfish is suspended in such a way that the oral-aboral axis is in the line of the current, the rays curve either orally or aborally so as to be pointed toward the cathode.

The direction of righting movements is easily controlled by the current, since the oral surface is turned toward the anode, to which, also, the tube feet extend. By reversing the current the progress of righting movements may be interrupted and caused to proceed in the opposite direction.

These results hold with *Henricia* as well as with *Asterias.* The orientations in the current are not reversed after immersion in solutions of caffeine, atropine, strychnine, veratrine, or nicotine. In certain cases, what appeared to be definite reversal of orientation of the rays followed immersion in pilocarpine solution (about 1:10,000); they swung about to the anode, not to the cathode. In other cases this was not so clear, although cathodally directed rays were seen to *shorten,* and to *elongate* upon reversal of the current. In both respects the normal behavior is just the contrary. The effect is of particular interest in view of the strychnine-like posture assumed with pilocarpine, as already described—although the tube feet retain attachment to the substratum (but no creeping is evident).

Injection of the proper amount of strychnine, as with *Leptosynapta,* produces both in *Asterias* and in *Henricia* aboral curling of cathodally directed rays, flat relaxation of anodal ones, with slight subsequent elevation from the substratum. The tube feet extend toward the cathode. The rays slowly swing into position with tips directed toward the anode.

In every respect this is complete reversal of the normal picture. Since the tube feet do not attach, or release attachment on stimulation by touch or by current, there is no progression toward either pole.

Pronouncedly diagrammatic effects of the current are also obtained with the serpent star *Ophiopholus aculeata.* With lower current densities (about 7 m.a. per sq.cm.), the arms all bend smoothly toward the cathode, resulting in a "step" toward the anode. The two rays nearest the anode, one on either side, then return somewhat to the radial position, then again swing cathodally. In this

way continuous creeping is maintained toward the anode. With higher current densities the arms remain immobilized in the extreme cathodal position, though quickly reversing when the current is reversed. The body of the central disk contracts radially on the anode side, expands on the cathode side. Isolated rays swing the distal tip sharply toward the cathode, and reverse with the current. As with the starfishes, the disk is tilted slightly so that the oral surface tends to face the anode. The direction of the orientation of the arms is not affected by immersion in solutions of atropine, strychnine, camphor, pilocarpine, picrotoxin, or nicotine. In strychnine the rays bend orally, particularly after stimulation; this is most pronounced at the tips of the arms. With nicotine oral-ward curling is more pronounced, from the disk outward; this is also true for a time in picrotoxin solutions. So long as the animals are capable of movement at all, the arms swing to the cathode—even with camphor treatment, which enormously accentuates their irritability. There is likewise no evidence of reversal of reaction to touch.

IV

The results show that by the action of strychnine (injected) it is possible to reverse the normally cathodic galvanotropism of *Leptosynapta, Asterias,* and *Henricia.* Other agents, so far as tested, failed to accomplish this (except in the case of *Asterias* with pilocarpine, which requires further study). In each positive case there is definite evidence of "reversal of inhibition" in the use of antagonistic muscle groups when responses are made to mechanical stimuli. These reactions involve contractions of the body musculature, and the associated reversals of reaction in constant electric current agrees with evidence from other forms previously studied. They are to be contrasted with the precise anodal turning of the spines, tube feet, and pedicellariae of *Strongylocentrotus,* which is not affected by strychnine. The movement of the spines involves local (nerve net) mechanisms, not the central nervous system proper, and their galvanotropic orientation persists in isolated portions of the test.

References

1. Bancroft, F. W. The control of galvanotropism in paramecium by chemical substances. *Univ. Calif. Publ. Physiol.,* 1906, **3**, 21-31.
2. ————. On the influence of the relative concentration of calcium ions on the reversal of the polar effects of the galvanic current in paramecium. *J. Physiol.,* 1906, **34**, 444-463.

3. ————. The mechanism of the galvanotropic orientation in volvox. *J. Exper. Zoöl.*, 1907, **4**, 157-163.

4. CARLGREN, O. Ueber die Einwirkung des constanten galvanischen Stromes auf niedere Organismen. *Arch. f. (Anat. u.) Physiol.*, 1900, **49**.

5. ————. Ueber die Einwirkung des constanten galvanischen Stromes auf niedere Organismen. *Arch. f. (Anat. u.) Physiol.*, 1900, 465.

6. CROZIER, W. J. The sensory reactions of *Holothuria surinamensis* Ludwig. *Zoöl. Jahrb. Abt. allg. zoöl. Physiol.*, 1915, **35**, 233.

7. ————. Galvanotropism and "reversal of inhibition" by strychnine. *Proc. Soc. Exper. Biol. Med.*, 1926, **24**, 282.

8. ————. Galvanotropism and "reversal of inhibition" by strychnine. *J. Gen. Physiol.*, 1926-1927, **10**, 395-406.

9. CROZIER, W. J., & STIER, T. J. B. The measurement of galvanotropic excitation. *J. Gen. Physiol.*, 1927-1928, **11**, 283-288.

10. FRIES, E. F. B. Drug action in galvanotropic responses. *J. Gen. Physiol.*, 1927-1928, **11**, 507-513.

11. HENRI, V. Étude physiologique des muscles longitudinaux chez le "Stichopus regalis." *C. r. Soc. biol.*, 1903, **55**, 1194.

12. HYMAN, L. H., & BELLAMY, A. W. Studies on the correlation between metabolic gradients, electrical gradients, and galvanotaxis. *Biol. Bull.*, 1922, **43**, 313.

13. MAST, S. O. Light reactions in lower organisms. II. Volvox globator. *J. Comp. Neur. & Psychol.*, 1907, **17**, 99.

14. ————. Light and the behavior of organisms. New York: Wiley; London: Chapman & Hall, 1911. Pp. xi+410.

15. ————. Reversion in the sense of orientation to light in the colonial forms, Volvox globator and Pandorina morum. *J. Exper. Zoöl.*, 1919, **27**, 367.

16. ————. Response to electricity in Volvox and the nature of galvanic stimulation. *Zsch. f. vergl. Physiol.*, 1927, **5**, 739.

17. MOORE, A. R. Reversal of reaction by means of strychnine in Planarians and Starfish. *J. Gen. Physiol.*, 1918-1919, **1**, 97.

18. ————. The action of strychnine and nicotine on the neuromuscular mechanism of Asterias. *J. Gen. Physiol.*, 1919-1920, **2**, 201.

19. MOORE, A. R., & GOODSPEED, T. H. Galvanotropic orientation in *Gonium Pectorale*. *Univ. Calif. Publ. Physiol.*, 1911, **4**, 17.

20. OLMSTED, J. M. D. The comparative physiology of Synaptula Hydriformis (Lesueur). *J. Exper. Zoöl.*, 1917, **24**, 333.

21. ROSE, M. Sur le galvanotropisme des *Gammarus* d'eau douce. *C. r. Soc. biol.*, 1927, **96**, 51.

22. TERRY, O. Galvanotropism of Volvox. *Amer. J. Physiol.*, 1906, **15**, 235.

Harvard University
Cambridge, Massachusetts

LE RENVERSEMENT DE GALVANOTROPISME CHEZ LES ÉCHINODERMES

(Résumé)

Les résultats de cette expérience montrent que par l'action de la strychnine (injectée) il est possible de renverser le galvanotropisme normalement cathodique de *Leptosynapta, d'Asterias,* et *d'Henricia.* D'autres agents, testés jusqu'ici, ne l'a pas accompli (sauf la pilocarpine, dans le cas *d'Asterias* ce qui exige des études plus étendues). Dans chaques cas positif il y a de l'evidence définie de "renversement de l'inhibition" dans l'usage des groupes de muscles antagonistes quand il y a des réponses aux stimuli mécaniques. Ces réactions comprennent des contractions de la musculature du corps, et les renversements associés de réaction avec un courant électrique constant s'accordent avec l'evidence chez d'autres formes étudiées antérieurement. Ils sont à contraster avec le tour anodal précis des piquants, des tubercules ambulacraires, et des pédicellaires de *Stronglocentrotus,* qui n'est pas influencé par la strychnine. Le mouvement des piquants comprend des mécanismes locaux (du réseau nerveux) et non du vrai système nerveux central, et leur orientation galvanotropique continue dans les portions isolées du test.

CROZIER

RÜCKWIRKUNG DES GALVANATROPISMUS BEI SEEIGELN

(Referat)

Die Resultate dieses Experiments beweisen dass man durch die Wirkung von Strychnin (eingespritzt) den normal kathodischen Galvanotropismus bei *Leptosynapta, Asterias,* und *Henricia* umkehren kann. Andere Agenzien hatten nicht dieselbe Wirkung (ausser Pilocarpin auf *Asterias,* was aber noch weiterer Experimente bedarf). In jedem positiven Fall fand man bestimmte Beweise von "rückwirkender Hemmung" im Gebrauch der antagonistischen Muskel Gruppen bei Reaktionen nach mechanischer Reizung. Diese Reaktionen führen Kontraktionen der Körpermuskulatur herbei, und die begleitenden Rückwirkungen der Reaktion bei konstantem elektrischem Strom stimmt überein mit Beweisen von andern Formen die man schon früher studiert hatte. Sie sollten verglichen werden mit der genauen anodalen Wendung der Stacheln, der Schlauchfüsse und der pedicellariae von *Strongylocentrotus,* auf die Strychnin keine Wirkung ausübt. Die Bewegungen der Stacheln bedingen örtliche (Nervennetz) Mechanismen, doch nicht das eigentliche Nervenststem, deren galvanotropische Orientation in eingelnen Teilen des Tests anhält.

CROZIER

TONE SHAPES: A NOVEL TYPE OF SYNAESTHESIA*

From the Psychological Laboratory of Wellesley College

MICHAEL J. ZIGLER

The attention of the writer has recently been called to the occurrence in the experience of two Wellesley College undergraduates of an unique type of synaesthesia in which the sound of each of several musical instruments is accompanied by a distinctive tridimensional form. The fact that musical sounds are associated with color impressions, photisms, bidimensional patterns, etc. in the experiences of certain persons has been reported in various places (8, 2, 10) since Goethe (4) first mentioned the existence of this type of synaesthesia. The writer has been unable, however, to discover anywhere in the literature descriptions of synaesthetic phenomena in which the patterns excited by musical instruments are clearly tridimensional in form.

The two synaesthetic subjects consented to come to the Psychological Laboratory to describe these "shapes" under conditions which we should arrange. Students possessing various musical instruments willingly agreed to bring them to the laboratory at appointed times to furnish the necessary stimuli. Some of the instruments were standard orchestral pieces, others were of less certain musical character. The instruments, in the order in which we used them, were: flute, saxophone, bugle, harmonica, jazz whistle, simplexophone, musical saw, cello, violin, and piano. The students with instruments were stationed in a large room and the two synaesthetic students in separate rooms adjoining the large room. A number of pitches at different levels within the playing range of the instruments were selected and played one after the other. The intensity of tones was varied in the three degrees, loud, medium, and soft. The duration of the sounds was varied from staccato to sustained. After the separate presentation of the different pitches at different intensities and durations, a familiar melody—in the case of the bugle, reveille and taps—was produced on each instrument. Chords were played on instru-

*Received for publication by H. P. Weld of the Editorial Board, December 21, 1928.

ments allowing that possibility, and duets were played on the flute and saxophone and on the violin and cello.

In no instance was the subject informed as to the instrument by which the tone or combination of tones was produced. We were, of course, unable to avoid the possibility that the subject might recognize the instrument by its peculiar timbre. Neither of the subjects manifested an inclination to name the instrument, however, nor did they remark whether the tones appeared novel or familiar. They appeared to be solely concerned with the task of observing the shape which the tone called forth. The investigation of one of the subjects was made by Miss M. Davidson, Graduate Assistant in Psychology, and that of the other by the writer. The colored forms were characterized by words and diagrams.

Both subjects were college sophomores. One was taking her first course in psychology, the other had had no work in academic psychology. It is therefore probable that the descriptions of these shapes are not entirely free of imaginative elaboration. Both Miss Davidson and the writer were impressed, however, by the promptness and spontaneity with which the descriptions were given, and by the evident care with which words were chosen and qualified so as faithfully to characterize the experiences. Both subjects are musical to the extent that they have played the piano from childhood, and one has played the violin also.

In a number of respects there is striking agreement in the reports of the two subjects in regard to the nature of the forms called forth by the sounds of the instruments; in many other respects there are points of great difference. The forms of the one are usually more vivid and more fully developed than those of the other subject. The former reports the surface characteristics of the shapes in great detail. The surfaces are characterized by such terms as hard, soft, velvety, jagged, uneven, smooth, polished, streaked, crystalline. In some cases there is a superficial layer of hard crystalline material which represents a crust or covering of an inner mass of soft jelly-like substance. At times there is a fairly definite surface of separation tween these two materials, at other times there is a gradual merging of the one into the other. Despite many variations in the mode of presentation of the notes, we were unable to find the determinative condition for the different kinds of surface character reported. The shapes of the other subject are seldom very vivid or fully developed.

Most commonly they are characterized as soft, fuzzy, hazy, foggy, unclear, poorly-defined masses. Although her forms possess a general shape or outline, her reports reveal few details of surface character. In contrast to the other subject, who states that her forms are more vivid than memory images but hardly as definite as perceived objects, this subject rates her forms not quite as vivid as her memory images. It appears that the two subjects represent distinctly different stages of definiteness of synaesthetic imagery. The one subject has forms which are almost as definite as her perceptual experiences; it is possible that they may be of an *eidetic* character (5, 1). For purposes of convenience in indicating points of difference in the tone shapes of the two cases, we shall henceforth designate that subject in possession of the more dominate type of synaesthesia, Subject A, and the other, Subject B.

Both subjects state that certain instruments characteristically give more definite and more clearly delineated tone shapes than others. Subject A, as might naturally be expected, gives evidence of wider range of degrees of definiteness than B. The stringed instruments give less definitely developed forms than wind instruments for both subjects. For both subjects, also, the tridimensional form tends to retain the same shape and general features under all variations of pitch, intensity, and duration. There is no apparent agreement, however, in regard to the specific form which belongs to any particular instrument. There is, it is true, a suggestion of similarity in the respective forms reported for the flute, which presents to A a thimble or acorn-cup-shaped figure and to B an elongated hollow tube. There is also some similarity perhaps in the simplexophone, which gives a megaphone shape for B and a dagger shape for A; but there is little, if any, resemblance in the forms excited by other instruments. Such extreme differences may occur as that which exists in the instance of the saxophone. For A the sound of this instrument excites a cup with a solid inner core, and for B the brusting and flying in all directions of particles of varied shapes.

The accompanying table indicates in a very schematic way the representative nature of the forms corresponding to the instruments used. Both subjects assert that the terms employed suggest meanings far too objective and familiar to characterize the experiences accurately. The following assigned objective labels are therefore intended merely to indicate the general shape of the perceived forms. B points

out in particular that her forms are so intangible and vaporous that the attempt to describe them verbally is at best very inadequate.

Instrument	Subject A	Subject B
Flute	Thimble or acorn cup	Hollow tube
Saxophone	Cup with solid inner core	Bursting of a mass into rough, jagged, and splintery particles.
Bugle	Morning glory or pipe	Sphere with opening on upper side
Harmonica	Series of spatially distributed discs	Flat rectangle
Jazz whistle	Thick waving streamer	Lumpy dough-like elongated mass
Simplexophone	Dagger	Megaphone of very vague outline
Musical saw	Elongated globule with jagged surface	Yards and yards of round ribbon-like material
Cello	Flat horizontal base with spring-like vertical projections	Thick ribbon
Violin	Tube with enlarged nodules	Ribbon much thinner and smaller than that of cello
Piano	Quadrangular blocks	Spheres

In both subjects, the higher pitches of all instruments are attended by smaller and usually by more solid, the lower pitches by larger and commonly hollow forms. B reports that, in the case of instruments which arouse spherical or roughly spherical shapes, the larger shapes excited by the lower notes tend to flatten on top while the smaller shapes of the higher notes retain a more nearly perfect spherical appearance. In those instances where the forms appear in colors, the higher pitches tend to give rise to lighter colors than the lower pitches.

Variation of intensity has much less influence than variation of pitch in the way of producing changes in the appearance of the shapes. We were unable to determine that variation of intensity produces any consistent change in the shapes for B. Subject A reports, however, especially in the case of wind instruments, that changes in definiteness vary directly with degree of intensity. She reports also that the weaker notes sometimes give "frailer" forms than the stronger notes.

In shapes which are elongated, the extent is a function of the duration of the note. In forms of roughly spherical nature, increase of duration adds to the number seen in succession during the rendition of the note. There are clear indications that these tonal shapes possess, in many instances at least, a dynamic character. This feature appears to be dependent also upon duration of tone. Both subjects frequently report that a series of spherical shapes pass across the field of vision in a definite direction. For A the members of a series pass from left to right from a position about 2 feet from the face to one of considerably greater distance. For B they tend to recede into the distance in the median plane of the body from a point about 4 feet from the eyes, where the shapes first appear. B sees one form after another in close succession, but perceives only one at a time. Subject A may see at the same time as many as three or four forms which vary in degree of definiteness and clearness.

Still other dynamic characteristics dependent upon the duration of the tone are manifested in elongated shapes. Sometimes there is merely a visible extension of an elongated form, at other times there is an apparent streaming or wave-like movement on the surface or even in the interior of a form which is undergoing elongation when the note is sustained. The forms are sometimes composed of viscous and jelly-like substances, and A reports an apparent "springiness" or elastic quality in certain forms. There were also sporadic reports by both subjects to the effect that the appearance of a form was delayed or retarded in development. Normally the mature form appeared promptly with the presentation of the note, but in these exceptional instances the form was assembled as if by the moving or streaming together of scattered small particles of variable sizes and shapes. Subject A reported incidentally that the unexpected sounding of an automobile horn near her sets up a cluster of small similar tridimensional shapes of slightly variable size that appear to issue from the horn. If the sound is sustained, a definite form develops, otherwise the particles disperse.

The complete development of the tonal shapes is also a function of duration. For A staccato presentation gives incomplete, poorly formed, cut-off, harsh, or unpleasant forms. B also states that this mode of presentation gives less definite forms than sustained presentation. Faulty production of a note sometimes results in a gap or break in the elongated form, or in an increased temporal interval

between successive spherical shapes; at other times extraneous accessory forms appear with the regular shapes.

A reports that the form appears simultaneously with the appearance of the tone; it comes spontaneously and is generally just as insistent as the tone. The two experiences—sound and tone shape—are however absolutely independent as mental phenomena. They show no tendency to overlap or to dovetail together. The forms appear to be only slightly, if at all, capable of being suppressed voluntarily. B, on the other hand, states that the tone is more intimately related to the instrument than is the corresponding tonal shape, which tends to appear quite promptly, but only if attention is directed to it.

The subjects are in striking agreement in regard to the rôle which color plays in the forms. Both report that shape is always primary, that color plays a secondary rôle only in these experiences. Some tonal shapes appear in colors, others are colorless. The colored ones are reported as aesthetically more pleasant than the non-colored. The absence of color does not hinder the maturation of a tonal shape in respect to all other details. These facts militate against the possible assumption that this type of synaesthesia may after all be merely a peculiar, atypical, or remote variety of colored hearing. Spectral colors seldom appear in the forms. Most frequently the hues are characterized by such qualifications as dark, light, gray, silvery, golden, tan, and brown. Aside from the fact that high pitches tend consistently to give lighter colors and lower ones to give shades or darker hues (11), color appears to be largely a fortuitous embellishment of the tonal shapes. Color does not appear to have any relationship with the type of instrument. Pitch is the only character which has a determinative influence, and most frequently the high notes were described as silvery or golden, and the lower ones as brown or tan. Notable exceptions to this, however, appear in the reports.

Familiar melodies arouse shapes of the same general character as those regularly excited by particular instruments. The size and apparent solidity of these shapes varies with pitch, as indicated in the foregoing description. B perceives tonal shapes easily in solo presentation only. In order to secure them at an orchestral concert, she must attend to a particular instrument and thereby ignore the accompanying tonal mass produced by the other instruments which

fail to generate tonal shapes. She also sees the shape of only one of the instruments of a duet. Subject A, however, perceives the specific forms which are aroused by both instruments of a duet united in such a way that the larger form occupies a lower position and appears to support the smaller one with which it is connected by a small thread-like projection. The smaller form is designated as an appendage of the larger one. In orchestral music she is able to perceive the forms of three or four instruments simultaneously, but the realization of forms under such circumstances appears to be somewhat dependent upon voluntary attention. This fact, although apparently at odds with her statement above that the forms appear spontaneously, is again suggestive of the relationship with *eidetic* imagery, which is alleged to require voluntary effort (7). Tones of very low clearness do not arouse tonal shapes in this subject, and her clearest shapes appear in solo presentation.

Both subjects state that the forms themselves apart from color possess a marked aesthetic appeal. Subject A, in particular, reports that they add greatly to her enjoyment of music. She indicates that, to a limited extent, the degree of pleasantness varies directly with the degree of definiteness in which the forms of particular instruments characteristically appear. The clarinet gives the best tone shape, and this is her favorite instrument. The tone shapes of wind instruments are for both subjects somewhat more pleasing, as well as more definite, than those of stringed instruments.

In order to determine whether or not the tone shapes are stable experiences, the subjects were required to describe again the forms excited by the same instruments at a later sitting. Subject A had transferred to the University of Kansas at the middle of the year; consequently we sent a copy of our manuscript to Professor R. H. Wheeler with the request that he or some member of his department undertake the reexamination of this subject under like conditions. It is with pleasure that we acknowledge the cooperation of Professor Beulah M. Morrison, who took the descriptions of this subject for the five instruments—flute, violin, cello, saxophone, and bugle. More than five months had elapsed since the subject described her forms in the Wellesley Laboratory. B also gave descriptions of the shapes aroused by all of the instruments used at the earlier sittings, but after an interval of three months. The results of both subjects show clearly that the tone shapes of these instruments remain stable in all important respects.

The earlier writers (7) were rather inclined to explain synaesthesia by reference to some neurological principle such as an anomalous anastomosis of nerve connections which gives a direct instead of the normal indirect connection between the cerebral centers involved. These writers, accordingly, were inclined to stress the hereditary source of the condition. Certain recent writers cite evidence, on the contrary, in support of the view that such experiences are probably acquired. Several of the early writers also inclined to the latter view (10). Mudge (11) has conducted a survey which shows that colored hearing occurs much more extensively than has hitherto been commonly supposed, and that it appears in different individuals in all degrees of definiteness. Wheeler (12) speaks of synaesthesia as "an immediate and permanent conditioned reflex." English (3) shows that the inception of colored hearing, in one child at least, can be traced to very early childhood.

The evidence is rather inconclusive as to whether the hereditary or the acquired theory best fits the facts presented in this study. Subject A reports that she has had tone shapes as far back as she can remember. B, however, has just recently noticed them. While attending an orchestral concert about five years ago, she suddenly made the discovery that vague, ill-defined forms appeared when she singled out the tone of the flute. She has since found that other instruments also give rise to shapes, and has become interested in these phenomena. The shapes have also improved somewhat in definiteness, and even in the course of our work of securing descriptions of the forms she reports a slight increase in definiteness. She is unable to report anything more definitely antecedent to this incident in listening to an orchestra that would indicate a gradual acquisition of these shapes, or any experience when tone and shape were fortuitously associated.

There are occasional indications of a crude and remote resemblance to the general shape of the musical instrument itself in the corresponding drawings of the subject. If this tendency were more pronounced, it would seem to point toward something like a very definite type of musical visual-imagery. In opposition to this suggestion, we fail to find any very striking agreement in the tone shapes of the two subjects for the same instrument. However, even in the face of many outstanding differences, the extent of agreement in the descriptions of the two subjects, especially in regard to the general conditions underlying the arousal of tonal shapes, is probably adequate to

justify the claim that we are here confronted by two instances of the same type of synaesthesia rather than by two fundamentally different types. Therefore, if the condition is hereditary in the one subject, it is probably not acquired in the other. On the other hand, it seems plausible that Subject A may have developed the tendency in early childhood, while B had the capability but did not discover it until relatively later and suddenly, much as the deaf subject Eugene Sutermeister (6) discovered, at an advanced age, his ability to appreciate music in terms of vibration sensations. The adherent of the hereditary theory might lay claim to the fact that remote relationships can be traced in corresponding shapes of the two subjects, and that the capacity in B in its initial manifestation was, compared with its present status five years later, relatively mature and did not pass through clear stages of development. But the exponent of that view would find it difficult to explain the diverse nature of the shapes of the two subjects for the same instrument, the delayed inception of the tendency in B, and the gradual improvement in the definiteness of the shapes of this subject.

Summary

We have reported two cases of a novel type of synaesthesia, in which the tones of different musical instruments give rise to correspondingly different tridimensional visual shapes. Every instrument excites a specific form, which maintains roughly the same features at all pitches, intensities, and durations. The higher pitches occasion smaller and lighter, the lower ones larger and darker shapes. The forms may appear in colors, but color is not always present and always plays a rôle secondary to that of form. These phenomena occur chiefly in solo rendition, and possess high aesthetic appeal. One subject has experienced the forms as far back as she can remember, the other discovered suddenly several years ago her capability of realizing them, and they have since gradually somewhat improved in definiteness. There are striking differences in the forms of the two subjects for the same instrument, but the general conditions of their arousal and alteration have many points in common. In view of this fact, the interpretation is made that the two subjects represent a single type of synaesthesia. Evidences for and against the hereditary and acquired theories, although somewhat favoring the latter view, are inconclusive. Relationship of these shapes to *eidetic* im-

agery is suggested in the almost perceptual character of the phenomena, especially in the case of Subject A, as well as in the fact that, for Subject B under all circumstances and for A under the more difficult conditions of their arousal, the shapes are realized under voluntary attention.

References

1. Allport, G. W. Eidetic imagery. *Brit. J. Psychol.*, 1924, **15**, 99-120.
2. Calkins, M. W. A statistical study of pseudo-chromaesthesia and of mental forms. *Amer. J. Psychol.*, 1893, **5**, 439-464.
3. English, H. B. Colored hearing. *Science*, 1923, **57**, 444.
4. von Goethe, J. W. *Theory of Colors*, 1810.
5. Jaensch, E. R. Ueber die subjektiven Anschauungsbilder. *Ber. d. VII. Kong. f. exper. Psychol.*, Jena, 1922.
6. Katz, D. Der Aufbau der Tastwelt. Leipzig: Barth, 1925. Pp. xii+270.
7. Kroh, O. Subjektive Anschauungsbilder. Göttingen: Vanderhoeck & Ruprecht, 1922. Pp. 95.
8. Krohn, W. O. Pseudo-chromaesthesia, or the association of colors with words, letters, and sounds. *Amer. J. Psychol.*, 1892, **5**, 20-41.
9. Langfeld, H. S. Note on a case of chromaesthesia. *Psychol. Bull.*, 1914, **11**, 113-114.
10. ————. Synaesthesia. *Psychol. Bull.*, 1926, **23**, 599-602.
11. Mudge, E. L. The common synaesthesia of music. *J. Appl. Psychol.*, 1920, **4**, 342-345.
12. Wheeler, R. H. The synaesthesia of a blind subject. *Univ. Ore. Publ.*, 1920, **1**. Pp. 61.

Wellesley College
Wellesley, Massachusetts

FORMES TONALES: TYPE NOUVEAU DE SYNESTHÉSIE

(Résumé)

Cet article est une enquête de deux cas de synesthésie où les tons de divers instruments musicaux sont accompagnés de formes de trois dimensions. Bien que ces formes ne sont pas les même chez les deux sujets, elles ont bien des ressemblances. Les formes sont en général élongées ou sphériques; celles-là deviennent plus grandes; celles-ci sans changement de grandeur, se mettant en mouvement dans le champ visuel; et dans les deux cas la quantité du movement est conditionée par la durée du son. La grandeur et la solidité des formes et aussi la clarté ou l'obscurité des couleurs des formes quand elles sont colorées, ce qui arrive de temps en temps, sont une fonction de la hauteur. La netteté des formes donne en partie une corrélation avec l'intensité des tons. Il y a aussi des différences individuelles. Chez un sujet les formes sont presque aussi nettes que les objets de

perception et à cet égard comme à d'autres égards montrent une ressemblance aux images éidétiques. Chez l'autre sujet les formes ressemblent à celles des images visuelles normales. Quand plusieurs instruments se jouent ensemble, l'un des sujets a toujours seulement une forme pour un seul instrument, tandis que l'autre sujet peut avoir des formes pour trois ou quatre instruments en même temps. Sans arriver à une conclusion définie, l'auteur est porté à croire que ces synesthésies on été acquises de bonne heure dans l'enfance.

ZIGLER

TONGESTALTEN: EINE NEUE ART DER SYNAESTHESIE

(Referat)

Diese Abhandlung ist eine Untersuchung von zwei Fällen von Synaesthesie wobei die Töne verschiedener Instrumente von Gestalten von drei Dimensionen begleitet werden. Obleich diese Gestalten bei den beiden Vpn. verschieden sind so gleichen sie sich doch in vielen Punkten. Die Gestalten sind gewöhnlich entweder länglich oder rund; die Ersteren nehmen àn Umfang zu; die Letzteren bewegen sich im Gesichtsfeld ohne grösser zu werden; und in beiden Fällen wird die Grösse der Bewegung durch die Dauer des Tons bedingt. Die Grösse und Solidität der Figuren, sowie deren Helligkeit oder Mattigkeit der Farben, wenn diese gelegentlich farbig sind, sind eine Funktion der Tonhöhe. Die Deutlichkeit der Gestalten korreliert teilweise mit der Stärke des Tons. Man fand auch individuelle Unterschiede. Für die eine Vp. waren die Figuren fast so deutlich wie wahrnehmbare Gegenstände und darin wie in andern Punkten ähneln sie der eidetischen Einbildung. Für die andere Vp. gleichen die Gestalten denen normaler Gesichtvorstellung. Wenn mehrere Instrumente zusammen spielen, so hat die eine Vp. nie mehr als eine Figur für ein Instrument, die Andere, dagegen kann gleichzeitig Figuren für drei oder vier Instrumente haben. Ohne irgend welche bestimmten Schlussfolgerungen zu ziehen, neigt der Verfasser zu der Ansicht, dass diese Synaesthesien in früher Kindheit angeeignet wurden.

ZIGLER

LOUDNESS AND TELEPHONE CURRENT*

From the Departments of Physics and Education of Westminster Training College, London

L. F. Richardson and J. S. Ross

Introduction

William James described Psychology as "the hope of a science." There would be more hope if psychic intensities could be measured.

The custom nowadays is to measure accurately stimuli and movements, but to leave psychic intensities quite unmeasured. This seems to us ill-balanced.

A recent important book by Dr. Harvey Fletcher (4*a*) describes a long series of thorough investigations into hearing made by the Bell Telephone Laboratories. Dr. Fletcher regularly uses the phrase "sensation-level" to denote a scale, the units of which are occasionally (Figure 71) called "sensation units" but more often "decibels." The decibel is defined (p. 69) so that when two acoustic waves have powers of J and J_0 microwatts, the former is to exceed the latter by $10 \log_{10} J/J_0$ decibels. From our point of view, the decibel is a special measure of stimulus, and not a unit of sensation; so that what Dr. Fletcher calls "sensation level" is not a scale of sensation, except perhaps at its zero point, which is defined to be the threshold. Fletcher (p. 226) further defines the "loudness" of any pure tone to be the number of decibels above the threshold, of a comparison tone of 1000 vibrations per second, which sounds equally loud. This is a mixed procedure, equality being judged psychologically, decibels measured physically. As such, Fletcher's scale of "loudness" is entirely different from the purely introspective scale used by our observer.[1]

A former research by one of us (8) depended chiefly on numerical mental estimates of the loudness of auditory imagery. Two eminent psychologists who read that work in draft found it difficult to believe that those numbers could have any genuine significance. In-

*Received for publication by Carl Murchison of the Editorial Board, October 5, 1928.

[1]Note added in January, 1930.

deed C. S. Myers (7, p. 252) had stated that sensations could not be measured. We have set out to explore this question by asking people to listen to telephones supplied in sequence with various measured electric currents or voltages.

The experimenter, in one room, regulated the current, signalled its onset, and measured its strength. The observer, in another quiet room, wore the headphones and wrote down his estimates of loudness. A pleasant loudness was first chosen as standard and called 1.00, and the corresponding standard current was applied before and after each unknown. The signals, made by a small electric lamp, were one flash for "standard begins"; two flashes for "unknown begins"; three flashes for "rest begins." The observer was kept in ignorance of the relative strengths of successive stimuli until the end of the last of his estimates as here published.

There is reason to believe (3, Figure 20) that, when the frequency is kept fixed, the current in a telephone is proportional to the voltage between its terminals and to the amplitude of the aerial wave produced; so the sound-energy per volume of air, being proportional to the square of the amplitude, is proportional to the square of the current and to the square of the voltage. The telephones chosen, Messrs. S. G. Brown's F type, had no adjustment; the sensitivity was fixed.

Current Generator

This is shown in Figure 1. It is of a well-known type (11, Figure 83). The particular valves, coils, and voltages were altered several times during the course of the experiments, but we always used 5 to 17 kilograms of copper in the tuning-coil τ_1, τ_2 and avoided iron except for a few woodscrews, the object being to avoid harmonics. Again the coils υ and σ were never closely wrapped round τ_1, τ_2, but only placed end to end. Accordingly the coupling was loose, and the frequency nearly independent both of the high tension and of the power output.

The condensor ι (Telegraph Condensor Co. 14, 1797, 177*) allowed the capacitance to be adjusted by steps of about 0.02 microfarad from 3.5 microfarads downwards.

Frequency Measurements

In the earlier experiments (March, April, May, and June) the telephone ω was clamped just above the water-tube resonator, and

TABLE 1

PARTICULARS OF GENERATOR

Ref. to Figure 1	Inductance henries Self	Mutual	Resistance for D. C. ohms	Ref. to Figure 1	Friodes
March					
υ	0.10		42.		
				ρ	Benjamin short-path redspot.
π_1, π_2	0.30		21.		
				ϕ	200-volt lighting mains.
σ	12.		2.3×10^3		
June and July					
υ	0.30	0.02	21.		
π_1, π_2	0.18	0.09	5.	ρ	Mullard P. M., 254.
σ	12.		2.3×10^3	ϕ	Batteries for this only.

the quarter wave-length observed, allowing for the end-correction. The corresponding frequency was found from Blaikley's (1, p. 549) value for the speeds of sound in dry air in tubes.

As the air was not dry, these values were checked in July by simultaneous measurements with a stroboscope ψ illuminated by a flickering neon lamp χ. The high tension battery ϕ was made just

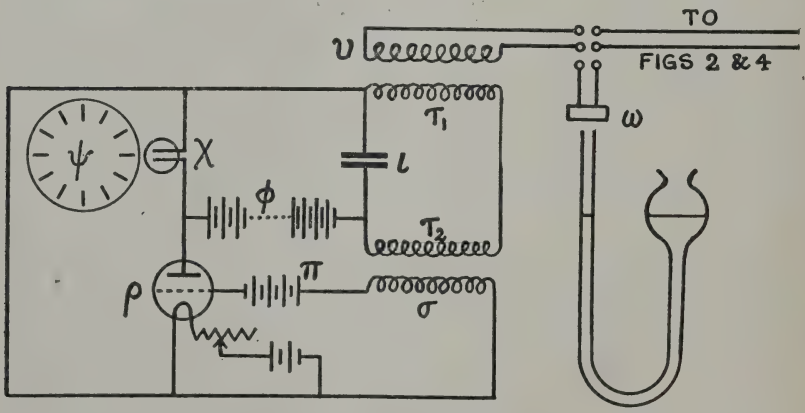

FIGURE 1

GENERATOR AND FREQUENCY METERS

insufficient to flash the lamp when the filament of the triode was cold. When this filament was heated, the oscillatory voltage of the coils, τ_1, τ_2, caused the neon lamp to flicker. The stroboscope had 100 and 360 holes and was turned by a gramophone motor (H. M. V.). The frequencies obtained in this way were self-consistent to about 1 in 500 and showed that the water-resonator values required no revision. Nevertheless in July we measured all frequencies by the stroboscope.

As a further check, the square of the reciprocal of the frequency was plotted against the capacitance, which had been measured by Mr. R. S. Maxwell, using a ballistic galvanometer, and by Messrs. J. R. Slater and C. V. Stephens, using a Carey Foster null method. The points were found to lie on a straight line, as theory indicates.

Harmonics. With loud sounds in the telephone the octave was heard faintly. Its presence was confirmed by the resonance tube; but the stroboscope did not show it. Very likely the octave is absent in the tuning coils, τ_1, τ_2, but is produced in the telephone as theory leads one to expect (6, p. 845). No other harmonic was noticed.

Test of Validity of Estimates in the Research on Conation, Imagery, and Cerebral Conductance

The Scale of Loudness. The scale of loudness was simply that which seemed intuitively natural to the observer (L. F. Richardson). As far as he could remember, it was the same as the scale which he had used during the preceding 10 months in estimating the loudness of inner imagery, as described in the paper aforesaid. The scale must have involved experience of measuring and numbering many other sorts of physical quantities. But as far as he is aware, it did not depend on any experience of physically measured sound intensities. For this particular experience happened to be scanty and amounted only to (*a*) a 2-hour experiment on noise made by rubber balls falling from measured heights (5 years ago) and (*b*) about 10 observations in November, 1926, on the telephone currents that produced sounds just audible, pleasant, and too loud. The observer took care never to listen to telephone currents changing in ratios known to him, during the period July 1, 1927, to June 15, 1928. The estimates of the loudness of inner imagery had been posted to America on May 2, 1928, so as to be quite free from sophistication by any knowledge of telephone currents.

Regulating and Measuring the Telephone Current. The arrangements are shown in Figure 2. When the observer was listening, the terminals at η were joined 2 to 3 and 1 to 4, leaving 5 and 6 disconnected.

a) The alternating electromotive force was applied to the telephone circuit by means of a variable mutual inductance ξ_1, ξ_2. The latter was of the Campbell type made by Messrs. R. W. Paul and believed to be subdivided to an accuracy of 1 in 1000 or better. The resistances and inductances of all parts of the circuit were measured. The well-known theory of the transformer (10, p. 274) then showed how the current depended on the mutual inductance. There are altogether three mutual inductances. But each of them is small relative to the geometric mean of the self-inductances of the same pair of coils. That is to say the couplings were all loose. Accordingly the telephone current would have been simply proportional to the variable mutual inductance were it not for the fact that the resistance and self-inductance of the telephone circuit varied automatically as more turns of wire were switched into ξ_2. A rather long calculation showed that

(current) varies as $M(1—bM)$

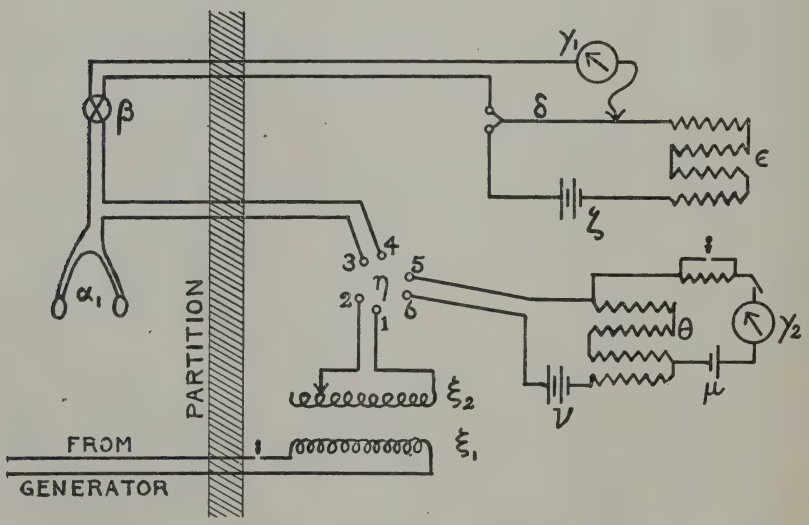

FIGURE 2

CURRENT REGULATION AND MEASUREMENT FOR 6-OHM HEADPHONE

where M is the mutual inductance in henries and b is a constant estimated to be roughly 15 henry $^{-1}$ in May and June.

b) *Absolute measurements.* About half of the telephone currents at each sitting were also measured absolutely by the vacuo-junction β in Figure 2. This was the most sensitive type sold by the Cambridge Instrument Co. and the headphones (Messrs. S. G. Brown's F type, 6 ohms) were selected of low impedance so as to require a current large enough to be thus measured. After each sitting the connections at η were changed by disconnecting 1 and 2 and joining 3 to 6 and 4 to 5. The voltage of the accumulator ν was measured in terms of the standard Weston cell μ and the accumulator ν was then used with the resistance box θ, having N.P.L. corrections, to supply known direct currents to the vacuo-junction taking account of the resistance of the heater and headphones. The electromotive force from the vacuo-junction was measured in March simply by a galvanometer, the scale of which was calibrated, as just described, at 21 points. But in May and June we preferred to balance the E.M.F. on the manganin slide wire δ which formed part of the potentiometer γ_1, δ, ϵ, ζ. The potentiometer was standardized after each sitting by the ν, θ, μ, γ_2 circuit. The "virtual" alternating current was taken to be that direct current which on the mean of the two senses of flow produced the same E.M.F. in the vacuo-junction as did the alternating current.

c) *Comparison of the two methods of measuring current.* The mutual inductance was very easily adjusted and read. It had a range of 1 to 10000 microhenries, which was ample. It was read on every occasion when the listener made an estimate, and is accurate.

The vacuum junction was wearisomely slow in arriving at a definite state; and occasionally it appeared afterwards that sufficient time had not been allowed. The galvanometer-zero drifted a little.

The fainter sounds corresponded to galvanometer-deflections that were too small to be measured. The vacuo-junction was used at every mental estimate of loudness in March and at about half of those at each sitting in May and June.

What we want mainly is the ratio between a pair of telephone currents occurring within about 15 seconds of one another and, as the most accurate way of obtaining this ratio, we fitted to the collection of absolute measurements by the vacuo-junction the formula

$$j = km\ (1—bm)$$

in which j is the "virtual" current in milliamperes, m is the mutual inductance in millihenries, and k and b are independent of m. Then if subscript 1 denotes the standard, the required ratio is

$$\frac{j}{j_1} = \frac{m(1—bm)}{m_1\ (1—bm_1)}$$

Or expanding by the logarithmic series

$$log_{10}\frac{j}{j_1} = log_{10}\frac{m}{m_1} — 0.434\left\{b(m—m_1) + \frac{b^2}{2}(m^2—m_1^2)\ \ldots\right\}$$

The observed values of b were

March 26—$b = 0.023$

March 27—$b = 0.037$

June 7—$b = 0.012$ } We took b as 0.015 for all May and June.

June 15—$b = 0.018$ }

In this way we consider j/j_1 is given with an accuracy of 2% or better whenever $j/j_1 < 1$ and an uncertainty not more than 10% at the worst. This is less than the uncertainty of the mental estimates.

Routine of Tests. The experimenter applied the standard currents before and after each test current. The tests were in a random sequence quite unknown to the observer. The observer made his estimate in the first 5 seconds after the unknown began, occasionally revising it on rehearing the standard. Telephones, if removed, were placed in the same position by feeling the lobes of the ears. On March 26 the observer could hear clicks when the inductance dials were turned. On all subsequent days, to prevent this clue, a switch was opened during the adjustment of the inductance.

Results. The best summary is Figure 3, but further particulars are necessary. For in the research on "Conation, Imagery and Cerebral Conductance" (8) the unit of loudness was taken to be the loudness of L. F. Richardson's ordinary inner speech, and faded imagery was recorded chiefly in the range 1.0 to 0.1 of that unit, excluding estimates of "zero." To fit this range on to the scale of telephone current he made comparisons in the course of the routine tests. These results are shown in the last three columns of Table 2.

From the mean values at the foot of each column it is seen that unfaded inner speech corresponded to $0.16/0.57 = 0.28$ of our standard current. The corresponding vertical line on Figure 3 is located on the abscissa at $log_{10}\,0.28 = \bar{1}.45$. This vertical corresponds

TABLE 2

Sitting	Number of estimates	Frequency per second	Virtual milliameters in S. G. Brown's F type 6 ohm headphones				
			Standard	Just inaudible	Phone relative to inner speech: Rather fainter	About the same	Rather louder
March 26	23	357	0.59		0.10	0.08	0.20, 0.16, 0.24
27	20	352	0.62		0.03	0.11	
May 31	12	354	0.77	0.010	0.06	0.32	
June 2	12	355	0.49		0.04		0.49
7	12	355	0.52	0.003		0.23	
9	12	356	0.51	0.006		0.09	
14	4	350	0.52			0.13	0.51
15	12	355	0.52		0.11		
	Means	354	0.57	0.006	0.07	0.16	0.32

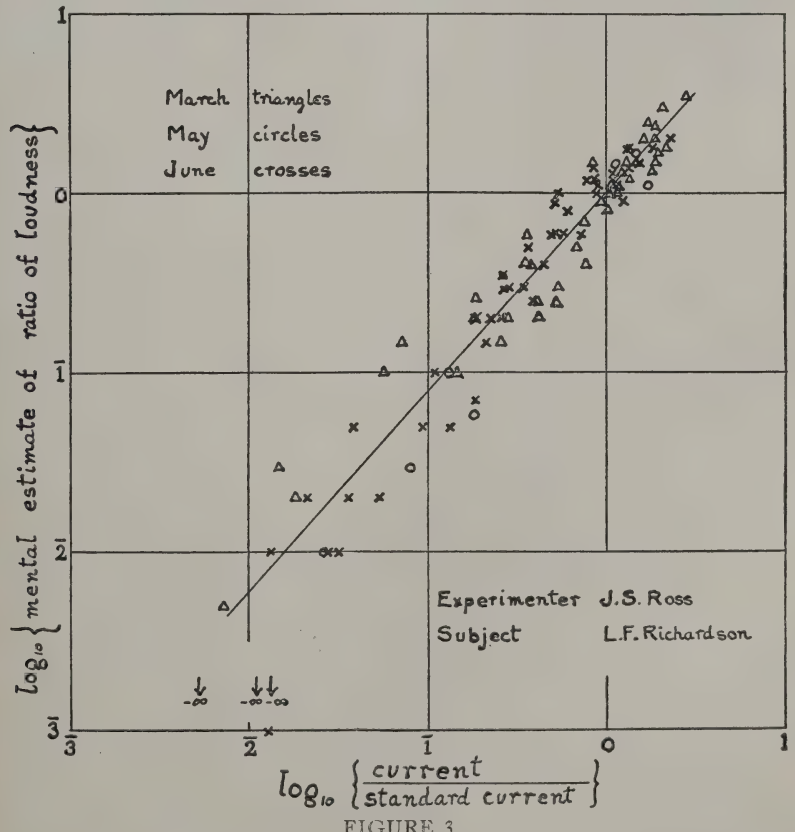

FIGURE 3

to a mean log_{10} (ratio of loudness) $= \bar{1}.40$. Therefore the range chiefly used in the research on imagery lies on the ordinate of Figure 3 between $\bar{1}.40$ and $\bar{2}.40$. This is a part of the diagram where the regression is linear, where the slope of the central line corresponds to the relation

$$(Loudness) = (Constant) \times (Current)^{1.1}$$

and where the mean deviation of log_{10} (current-ratio) from the central line is found to be 0.19, corresponding to an uncertainty of the current in the ratio 1.6. This uncertainty is sufficiently small to justify the *mean* values of the work on imagery.

Difficulty in estimating loudness seems to increase (*a*) with absolute faintness, but also (*b*) with greater separation from the standard. As in the research on imagery, the standard was nearer, the uncertainty may be a little less than that just calculated.

Apart from zero, only two estimates in that research fell below the range just considered; they were 0.05 and 0.02 of the loudness of normal inner speech, and on Figure 3, the corresponding points are located on the ordinate at $\bar{2}.10$ and $\bar{3}.70$. It is difficult to know whether the regression is still linear in this region, for what is just inaudible depends on the quietness of the room.

Quick Tests of Several Persons

Regulating and Measuring Voltage. Taking a hint from the

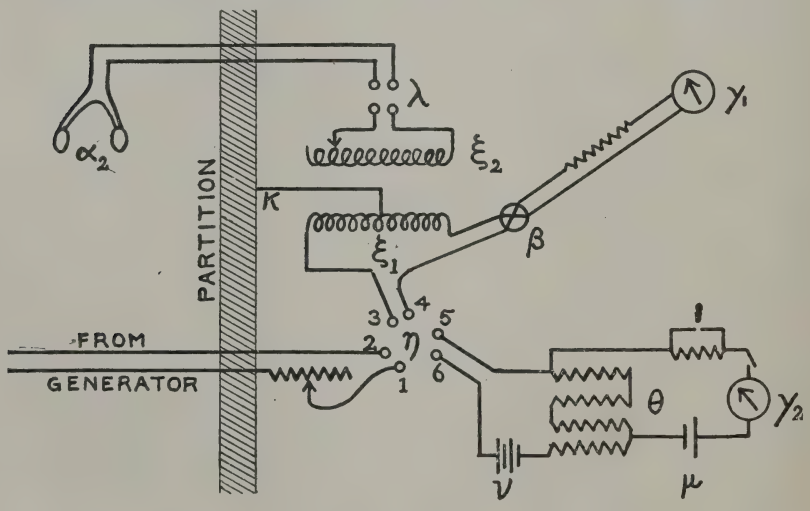

FIGURE 4

Voltage Regulation and Measurement for 4000-Ohm Headphone

sound division of the National Physical Laboratory, we rearranged the apparatus as shown in Figure 4. The torpid vacuo-junction β was placed in the primary circuit ξ_1 of the inductometer, and was there used to see that the primary current was kept constant, and to measure that constant. Telephones a_2 were chosen having so large an impedance that in comparison we could neglect the variation of the impedance of the secondary of the inductometer, a variation which caused the *b*-complication mentioned on page 292. The standardizing circuit ν, μ, θ, γ_2 is shown in both Figures 2 and 4, as it was used with both arrangements.

The telephones and the inductometer were 5 meters from each other and the nearest was 10 meters from the generator. A large search coil was used to make sure that stray fields were negligible.

With the telephone of large impedance a sound was heard even when the mutual inductance of ξ_1, ξ_2 was zero. The sound persisted when one telephone terminal was disconnected, but ceased when both were disconnected. The telephone being connected again, the sound was abolished when a wire from the midpoint of the primary of the inductometer was held in the mouth of the person who wore the headphones. As some people might dislike a wire in the mouth, the midpoint of the primary ξ_1 was connected instead by κ to a waterpipe which was almost as good. This stray current behaves as if it were due to two capacitances in series (*a*) between the coils of the inductometer and (*b*) between the telephones and the head. It sometimes made a sound as loud as that due to 30 microhenries of mutual inductance.

The effective voltage applied to the headphones is calculated as (*effective current in primary*) 2π (*frequency*) (*mutual inductance*) thus ignoring any harmonics. They are known to be very weak.

The "voltage-ratio," defined to mean (test voltage) / (standard voltage), is thus simply equal to (test mutual inductance) / (standard mutual inductance).

Routine. The routine for the experimenter was as follows. Double pole switch λ (in Figure 4) opened; ξ_2 set at standard, galvanometer γ_1 read; one tap on signalling key; λ closed; waited 15 seconds; λ opened; set ξ_2 to a different value; two taps on signalling key; λ closed; waited 15 seconds during which the mutual inductance was recorded; then the cycle began again.

To the observer seated in the adjoining room this routine appeared as: silence, one flash; standard sound during 15 seconds, 10 to 15 sec-

onds' silence, two flashes, a different sound the loudness of which must be estimated, 10 seconds' silence, one flash; standard sound again, and a second estimate of the previous unknown.

Specimen observation. Observer Professor J. C. Flügel, the well-known psychologist. Various pitches over a range of 3 octaves were offered, and he chose one. Frequency 1100 per sec. Eleven preliminary tests were disregarded at the observer's request. Millivolts across telephone were 2.5 when inductance was 100 microhenries.

Serial number	Mental estimates: When unknown began	Mental estimates: When standard began again	Observer's notes	Mutual inductance microhenries (standard = 100)
12	20.0	10.0		6000
13	0.2	0.4		6
14	4.0	2.5		600
15	?	0.5		60
16	20.0	15.0		10,000
17	0.05	0.10		2
18	2.5	2		100
19	7.0	7.		1000
20	0.2	0.4		10
	interval for luncheon			
21	1.4	1.6		150
22	5.0	4.0	interval seemed long	1500
23	0.25	?		1.5
24	?	0.25	surprised	3
25	8.0	10.0		3000
26	3.0	2.0		300
27	0.0	0.5		30
28	15.0	15.0		10,000
29	0.05	0.2		10
30	1.0	1.0	surprised	101
31	0.005	0.01		1
32	0.1	0.2		10
33	3.0	3.0		1000
	a few minutes rest			
34	0.1	0.2		3
35	0.02	0.05		1.5

Results. In Figure 5 the coordinates have the following meaning:
$X = log_{10}$ (test voltage) / (standard voltage).

$Y = log_{10}$ (ratio of loudness to standard)

Circles show estimates made at the beginning of the test sound, crosses show those made at the beginning of the subsequent standard. Where cross and circle coincide the effect is like a black dot.

In each diagram the louder part to the right of the center is

approximately straight. The slope of a well-fitting straight line, and the mean deviation from that line, are set out in Table 3.

Explanation. n is the slope of a straight line that fits well the diagram of the type of Figure 5 for the louder sounds, namely, for half the diagram. For this region loudness = (constant) × (voltage)^n.

Δ is the mean deviation of $\log_{10}$ (loudness ratio) from the aforesaid line for the same part of the diagram. In forming this mean,

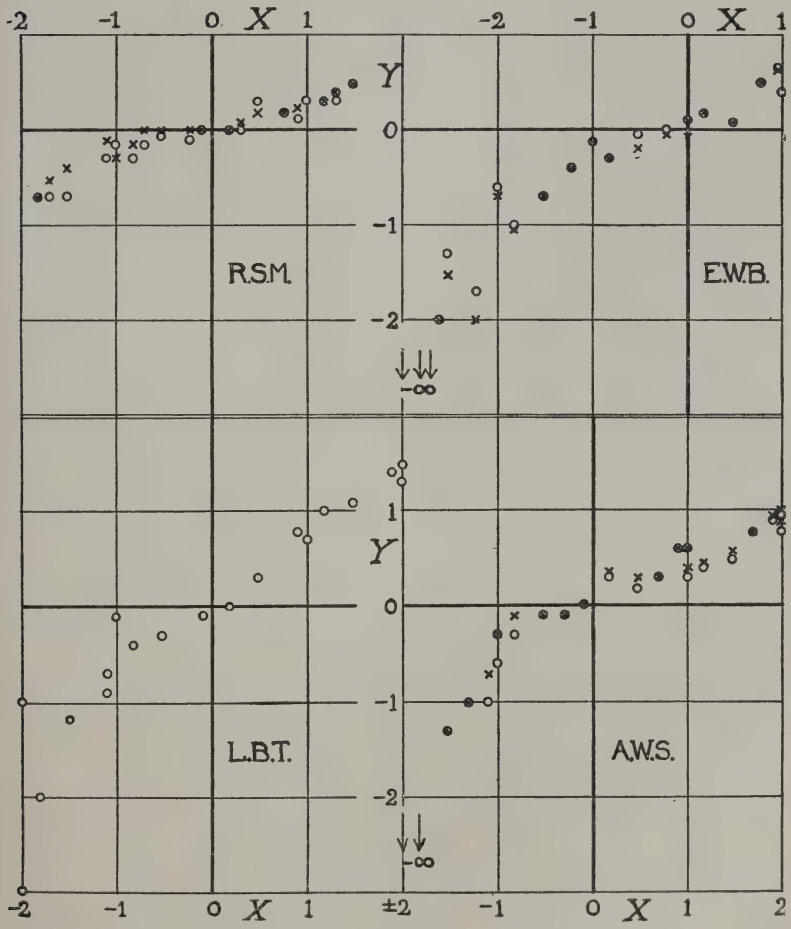

FIGURE 5

LEGENDS X AND Y ARE DEFINED ON PAGES 299-300

the first and second estimates of the same test sound were counted as two if they differed, as one if they coincided.

Y_0 is the intercept made by the same straight line on the Y axis.

TABLE 3

SUMMARY OF OBSERVATIONS USING S. G. BROWN'S F. 4000-OHM HEADPHONES

Date 1928	Observer	Age	Sex	Effective electromotive force: Just unpleasantly loud (volts)	Standard (millivolts)	Just inaudible (millivolts)	n	Δ	Y_0	Frequency per sec.
July										
10	R. S. Maxwell		M	0.08	2.5	0.03	0.24	0.06	0.04	765
13	E. O. Martin		M	?	2.4	?	0.40	?	?	1108
17	E. O. Martin		M	?	2.4	$<$0.04	0.65	0.16	0.18	1108
13	A. W. Spurr		M	0.2_0	2.5	0.04	0.38	0.10	0.10	550
13	L. B. Trant		M	0.2_0	2.4	$<$0.03	0.73	0.07	0.00	1108
13	J. S. Ross	35	M	0.2_1	2.4	$<$0.03	0.55	0.10	−0.18	1108
16	E. W. Barrett		M	$>0.2_4$	21.3	0.04	0.40	0.10	0.12	765
17	J. C. Flügel	44	M	?	2.5	$<$0.03	0.53	0.11	0.14	1108
20	R. Aspinwall		M	0.2_1	2.4	?	0.26	0.06	0.00	550
27	P. C. Austin		M	0.1_2	2.1	$<$0.02	0.34	0.07	0.00	1100
Aug.										
17	C. S. Myers	55	M	?	1.9	$<$0.02	0.37	0.06	0.05	767
Means							0.44	0.09	0.04	

Fechner takes "stimulus" to be proportional to energy, that is, to j^2, where j is the telephone current.

If, with Fechner, we suppose that

$$l = k \log_{10}(j^2/a)$$

l being loudness and k and a being constants and l_0, j_0 the standard values then

$$l_0 = k \log_{10}(j_0{}^2/a)$$

whence it follows that

$$l/l_0 - 1 = \frac{2k}{l_0} \log_{10}(j/j_0)$$

On plotting l/l_0 against $\log_{10}(j/j_0)$, the last formula indicates a straight line, but actually we find a curved distribution for the sub-

jects Richardson, Maxwell, Trant, Barrett; and presumably for all, for the above Fechnerian formulae imply that

$$log_{10}(l/l_0) = log_{10}\left\{1 + \frac{2k}{l_0} log_{10}(j/j_0)\right\}$$

that is, in the coordinates of Figure 5,

$$Y = log_{10}\left\{1 + \frac{2k}{l_0} X\right\}$$

This is a curved locus, but actually we find for all subjects that the (X_1Y) graph is practically straight when X is not too small.

Replies to Objections

W. James (5) quotes Stumpf: "One sensation cannot be a multiple of another. If it could, we ought to be able to subtract the one from the other, and to feel the remainder by itself. Every sensation presents itself as an indivisible unit" (p. 547). This true remark of Stumpf's lies dangerously near to falsity, as may be seen from the following parody: "One mountain cannot be twice as high as another. If it could, we ought to be able to subtract the one from the other and to climb up the remainder by itself. Every mountain presents itself as an indivisible lump." In the parody we have inserted a confusion between a concrete thing, mountain, and its measurable quality, height. There is risk of a similar confusion with sensations. Sensations cannot be subtracted, but the numbers representing their intensities can.

As tending to prove that sensations cannot be measured, it has been asserted by C. S. Myers (7) that: "The cannon roar is louder than the crack of a pistol, but we cannot express the former experience in terms of the latter" (p. 252).

Although our opinion is not based on special experiments with cannons and pistols but only on memories of the war, we are inclined to agree with Myers. But our reasons are special, namely, (*a*) that the roar of a cannon is temporarily deafening to anyone standing near it; (*b*) that the roar differs from the crack not only in loudness but also in duration, in mean pitch and in intense unpleasantness; and that these other differences distract attention from loudness. There is nothing in these special difficulties which can be regarded as an objection to the estimation of loudness in favorable circumstances.

The Logic of Measurement

Dr. N. R. Campbell in a recent analysis of the logic of measurement (2) divides magnitudes into classes A and B:

A-magnitudes are those which are measured by physical processes resembling the addition of equal numbers; for example by the placing of meter-rods end to end, or by the putting of equal weights into the same pan of a balance, or by the connecting of equal electric resistances in series.

B-magnitudes are those which are derived from A-magnitudes by calculation.

We have to consider whether the mental estimates fit into Campbell's classification. One observer, L. B. Trant, who is the college organist, reported that he "thought of how many organ-pipes would produce a sound like that." C. S. Myers, after estimating four unknowns, said, "How I do it, I don't know"; but after further introspection he recorded, at the 12th unknown, that he thought of "the strength of the stimulus to produce such a difference"; at the 18th, that he "imagined two equal sounds"; at the 20th, that he "imagined five sounds together." No other observer reported anything like addition. Most failed to describe the way in which they arrived at their numbers. J. S. Ross "hazarded guesses." L. F. Richardson "could not detect (*a*) any mental addition of imaged loudnesses, (*b*) nor any calculation." For him certainly, and perhaps for most of the others, the mental estimates are neither A-magnitudes nor B-magnitudes in Campbell's sense.

Fechner (4, p. 60) assigned numbers to sensations by counting increments in some sense equal. The scale of sensation, thus numbered, would be an A-magnitude, if it conformed to the other laws of measurement as set out by Campbell.

Summary

1) Of 11 persons tested, none has refused to assign numbers to loudnesses, though several found it difficult or complained that they were "only guessing."

2) Sounds that were unpleasantly loud were produced by telephone currents that were about 10,000 times stronger than those which produced sounds almost inaudible. The corresponding sensations usually vary in a less ratio, but for some subjects by as much as 1000. Because these ratios are so great, it is convenient to plot logarithms of current j and of loudness l.

3) The points on the (*log j, log l*) diagram for any one person, although scattered, lie conspicuously near to a smooth curve.

4) The shape of this curve has the following features common to all the persons tested:

a) For moderate and loud sounds it is nearly straight, implying that (loudness) = (constant) × (current)^n.

b) For fainter sounds the slope $d \log l/d \log j$ becomes greater except for two observers (C. S. M. and L. F. R.).

c) A portion of one branch of a hyperbola, having one asymptote inclined with a slope nearly equal to n, and the other parallel to the axis of *log l*, would fit many observations well.

d) For inaudible sounds $\log l \to -\infty$, while *log j* may vary in a large range. There is thus an infinite gap between groups of observed points. We have not investigated the threshold in detail, but it is probably "confused by a vagueness, uncertainty, or scatter" (9).

5) Persons differ considerably in the index n. It has ranged from 0.24 (for R.S.M. in Table 3) to 1.1 (for L.F.R. in Figure 3), with a mean of about 0.5. That is to say, a telephone current 10 times stronger than the standard is, on the average, judged to produce a sound three times as loud, but the extreme estimates are 1.7 times and 13 times as loud as the standard. Two explanations are possible. Suppose two fixed telephone currents, the same for all persons. Then we may have either (*a*) sensations the same for all persons, divergent estimates wrong, or (*b*) sensations really different for different persons, estimates showng this fact.

We do not know which of these two explanations, (*a*) or (*b*), is the more correct.

6) The scatter of the estimates of loudness is measured by a mean deviation of 0.09 in $\log_{10} l$ from the best fitting curve, for all sounds that are not too faint.

7) Two observers imagined stimuli in superposition. But for one observer at least, and perhaps for most, the loudness is neither an A-magnitude nor a B-magnitude in the sense that these terms are used by N. R. Campbell.

8) Sensation, as Fechner numbered it, is perhaps an A-magnitude. There is no reason to expect Fechner's scale to agree with the present intuitive scale. And if, in conformity with Fechner (4, pp. 20-40; 175-182), we measure the stimulus by the density of sound-energy per volume, we have not found any subject for which

(intuitively estimated sensation) $=$ k *log* (stimulus-constant) where k is a constant. Whether Fechner's law is true for the scale defined by him is a separate question.

9) The decibel scale of "loudness" as defined by Dr. Harvey Fletcher (12) is not a scale of sensation.

10) Persons insensitive to frequency-differences ("tone deaf") can estimate loudness with no more scatter than those who have good musical ears (see Figure 5 in which R.S.M. and L.B.T. are sensitive to frequency differences, while E.W.B. and A.W.S. are insensitive).

11) The observations in the section on Test of Validity of Estimates in the research on "Conation, Imagery, and Cerebral Conductance" (8) tend to justify the averages of the previous estimates of L. F. Richardson on auditory imagery by showing that the mean uncertainty of an individual estimate is in the ratio 1.6.

12) We are indebted to Mr. F. C. Bartlett for the suggestion that it was desirable to make experiments on sensations. Mr. Albert Campbell freely gave up his expert advice on alternating currents. We wish to thank the observers for their care in making estimates.

References

1. Blaikley, —. In Barton Textbook of Sound. London: Macmillan, 1922. Pp. 687.
2. Campbell, N. R. Measurement and calculation. London: Longmans, Green, 1928.
3. Cohen, B. S., Aldridge, A. J., & West, W. *J. Instit. Elec. Eng.*, 1926.
4. Fechner, G. T. Elemente der Psychophysik. Vol. 1. Leipzig, 1860.
4*a*. Fletcher, H. W. Speech and hearing. London: Macmillan, 1929. Pp. xv+331.
5. James, W. Principles of psychology. Vol. 1. New York: Holt, 1901.
6. Jewett, F. B. Article telephony. In Dictionary of Applied Physics. Vol. 2. London: Macmillan.
7. Myers, C. S. Text book of experimental psychology. Vol. 1. London: Longmans, Green, 1925.
8. Richardson, L. F. Imagery, conation, and cerebral conductance. *J. Gen. Psychol.*, 1928, **2**, 324-352.
9. ————. Threshold when sensation is regarded as quantitative. *Brit. J. Psychol.*, 1928, **19**, 158-166.
10. Russell, A. Alternating currents. (2nd ed.) Vol. 2.
11. Turner, L. B. Outline of wireless. Cambridge: Cambridge Univ. Press, 1921.

Technical College
Paisley, Scotland

Westminster Training College
London

L'INTENSITÉ DES SONS ET LE COURANT TÉLÉPHONIQUE

(Résumé)

On a fait cette étude dans le but de constater (1) si l'on peut faire des estimations numériques de l'intensité, et, cela étant possible, (2) le rapport fonctionnel entre l'intensité moyenne estimée et le courant téléphonique, (3) la constance des estimations successives d'un individu, et (4) la différence entre les individus dans leurs estimations.

L'expérimentateur, dans une chambre, a réglé un courant produisant une note musicale d'une fréquence constante et d'une intensité variable, a signalé son approche au moyen de lumières, et a mesuré sa force; pendant que l'observateur, dans une autre chambre, a écouté un appareil téléphonique, et on lui a demandé d'ecrire, si possible, des estimations numériques de l'intensité des sons du test comme multiples de l'intensité d'un son étalon, sonné avant et après chaque test. Le courant téléphonique a subi des variations entre 10.000 et 1.

Les réponses aux questions citées en haut ont été celles-ci:

(1) Tous les onze observateurs ont réussi à donner des numéros aux intensités.

(2) Pour les sons qui n'ont pas été trop faibles,

$$(\text{intensité}) = \text{constant} \times (\text{ou courant ou voltage})^n$$

Pour les sons faibles, l'intensité a été généralement moindre que celle trouvée par extrapolation de cette formule appliquée aux sons de grande intensité.

(3) La constance des estimations individuelles, mesurée par "l'écart moyen" $\Delta \log_{10}$ (intensité) de la courbe individuelle moyenne, mise en moyenne $\Delta = 0{,}09$ pour la partie de plus grande intensité.

(4) n a varié de 0,24 à 1,1 avec une moyenne de 0,5.

Pour l'observateur (L. F. R.) de l'article sur "Conation, Imagerie et Conductance Cérébrale," et pour les sons faibles notés là, $n = 1{,}1$: et l'écart moyen de $\log_{10}$ (courant) de la ligne moyenne a été de 0,19 pour une intensité fixe.

RICHARDSON ET ROSS

TONSTÄRKE UND DER TELEPHONSTROM

(Referat)

Der Zweck dieser Untersuchung war zu entdecken (1) ob man die Tonstärke in Zahlen abschätzen kann, und (wenn dies mölich ist) (2) das funktionelle Verhältnis der mittlern also abgeschätzten Tonstärke zu dem Telephonstrom, (3) wie konsequent eine Vp. in solchen auf einander folgenden Abschätzungen wäre, und (4) in wie fern die Vpn. variieren oder übereinstimmen.

Der VI., in einem Zimmer, regulierte einen Strom der einen musikalischen Ton von konstanter Häufigkeit doch variieren der Instensität hervorbrachte, signalisierte dessen Ansatz durch das Licht und bemass dessen Stärke; während die Vp., in einem andern Zimmer, am Telephon horchte, und wenn möglich die Tonstärke in Zahlen aufzeichnen sollte als Multiplum der Tonstärke des Masstabs, der vor und nach jedem Test angegeben wurde. Der Telephonstrom variierte von 10,000 bis 1.

Die Antworten auf diese Fragen waren wie folgt:

(1) Allen 11 Vpn. gelang es die verschiedenen Tonstärken in Zahlen auszudrücken.

(2) Für Töne die nicht zu schwach waren (Tonstärke) = Konstant x (entweder Strom odor *voltage*)^n. Für schwache Töne war die Tonstärke gewöhnlich geringer als die welche durch die Extrapolisierung nach dieser Formel den lauten Tönen angepasst waren.

(3) Persönliche Konsequenz, bemessen nach der "mittlern Abweichung" von Δ in $\log_{10}$ (Tonstärke) von der persönlichen mittlern Kurve, im Durchschnitt $\Delta=0.09$ für den lautern Teil.

(4) n erstreckte sich von 0.24 bis 1.1 mit einem Durchschnitt von 0.5. Für den Beobachter (L. F. R.) der Abhandlung über "Antrieb, Einbildung und Gehirnleitung" und dür die schwachen Töne die dort erwähnt werden, gibt $n = 1.1$; und die mittlere Abweichung des $\log_{10}$ (Strom) von der mittlern Linie was 0.19 für eine bestimmte Tonstärke.

RICHARDSON UND ROSS

SHORT ARTICLES AND NOTES

EDITED BY

F. C. Bartlett
V. Borovski
F. Kiesow
Matataro Matsumoto
Walter Miles
W. R. Wilcocks

DIFFERENTIAL ADAPTATION OF THE TWO SIDES OF THE RETINA

Dorothea E. Johannsen and Mason N. Crook

Introduction

In a study of light-spot adaptation, Dunlap (2) reported greater rapidity of adaptation on the nasal than on the temporal side of the retina. When a part of this experiment was repeated in the Clark Laboratory (1) the same phenomenon was incidentally observed though no data were recorded. Therefore, a short series of observations preliminary to the main experiment was made with one observer (M. N. Crook, C), in order to obtain quantitative confirmation of the reports. The light adapted out, on the average, .73 second more quickly on the nasal than on the temporal side of the left eye, and 2.52 seconds more quickly on the nasal than on the temporal side of the right eye. Dunlap had suggested that the phenomenon was probably due to more eye-movement when the spot fell on the temporal than when it fell on the nasal side of the retina. The observers in the Clark experiment, however, had reported an impression that the difference was not due to eye-movement; the spot which fell on the temporal side appeared brighter. Therefore, the apparatus was set up in the present experiment in such a way as to eliminate the possibility of differential eye-movements and, under these conditions, substantial differences were obtained.

Apparatus

The experiment was performed in a dark room. The source of light was a 25-watt daylight bulb, enclosed in a light-proof box, the interior of which was lined with white asbestos. In the front of the box was a ground-glass screen, next to which was placed a brass diaphragm with a round aperature, 1 cm. in diameter. The box was placed in the median plane,

the light spot directed away from *O*. The spot was reflected from two mirrors so placed that an image of it fell on each side of the retina at a visual angle of 10° from the fovea. An artificial pupil, 2 mm. in diameter, was used. The distance from the artificial pupil to the mirror was 139.9 cm., and the distance from mirror to light was 50.8 cm., making a total distance of 190.7 cm. A red cross of low intensity was used for fixation; the vertical arm was 1.5 and the horizontal arm 1.9 cm. long, and the cross stood at a distance of 49.8 cm. from the artificial pupil. Both cross and light spots were at eye-level. Stimulus light and fixation-cross were controlled by *E* from a switchboard. Adaptation time of the two spots was recorded by two stop-clocks which were started by electromagnets; the magnets were excited simultaneously by the switch which turned on the light. Either clock could be stopped independently by an electromagnet which *O* controlled through a finger-key.

Observers

The observers were C. Graham (G), L. Harden (H), R. Leeper (L), and R. Stone (S), all graduate students in the Department of Psychology. H was the only one having previously observed in vision experiments other than those of a general laboratory course.

Procedure and Results

An observation consisted of 15 to 30 trials with each eye, the number depending on the *O;* one trial involved adaptation of a spot on each side of the retina. Instructions to observers were as follows:

"Observe with the two eyes alternately, starting with the right or left as the experimenter shall stipulate. Place the eye at the eye-piece, fixate the center of the cross and hold the head as motionless as possible against the forehead and chin-rests. At *E*'s "ready-now" signal you will be shown two light-spots which are equidistant from the fixation cross. If only one spot appears at *E*'s signal, report the fact verbally and shift to the other eye. Immediately before you are two finger-keys, one for each hand. Keep the hands in position to signal with the keys without delay. If two spots appear, maintain fixation and observe the spots. If either spot disappears, or if both disappear either simultaneously or successively, report at once by means of the corresponding finger-key, using the left key for the left spot and the right key for the right spot. The series will be preceded by a 10-minute period of dark adaptation."

After a few seconds' observation each spot was surrounded by a luminous haze, and the criterion of extinction was the loss of differential outline and brightness between the spot and the haze.

The observer was instructed to report if only one spot appeared in the field in order to make sure that proper registration with the artificial pupil was obtained. Very few false starts were made after the practice series. When either key was pressed the corresponding clock was automatically stopped, and *E* read the time to ¼ second. When both clocks

had been stopped, *E* turned off the stimulus light and recorded the readings while *O* had a few seconds for dark adaptation. At the switch-board *E* used a small flash-light which was carefully screened from *O*'s field of vision.

Series I consisted of 200 trials for each *O*, under the conditions specified. Series II differed from I only in the following respects: (1) it consisted of 65 trials, and (2) the right spot was reported with the left key and the left spot with the right key instead of vice versa; thus any possible effect of differential reaction-time of the *O* or differential lag in the apparatus was controlled. The results of Series I and Series II are presented in Table 1.

We have arbitrarily called a difference plus (+) if the nasal side of the retina adapted more quickly than the temporal, and minus (—) if the temporal side adapted more quickly than the nasal. The differences are consistently in the plus direction for three *O*'s and in the minus direction for one *O*; of the 16 differences, 10 are highly reliable. Any difference between Series I and Series II is apparently an individual matter and, in any case, is insufficient to overcome the prevailing direction tendency.

Observers L and S wore glasses; this increased the difficulty of maintaining registration with the artificial pupil, and so each of these *O*'s was given Series III, which consisted of 100 trials without glasses. The results are recorded in Table 2. Series III is consistent in direction with Series I and II. S's results are less reliable than in the preceding series, probably because of greater diffusion due to astigmatism. This hypothesis is in accord with the results from a series of 50 trials with glasses but without the artificial pupil, which was given observer S. The data for this series (IV) are recorded in Table 2. Here we find the only inversion in the direction of the difference for any *O*. Without the artificial pupil, diffusion was not only greater, but the light was brighter, necessitating longer fixation. S had had no practice under these conditions, and it may be mentioned that S was the most erratic *O* in the practice trials for Series I.

Summary of Results

In Table 3 are summarized all the reliabilities in terms of chances in 1000. All but one of the 20 differences are consistent in direction with all the others for the same *O*. The most experienced *O* shows the most reliable differences. These results indicate that there is a real difference in the adaptation time of the two sides of the retina.

Discussion

Two possible alternative explanations of the above differences are suggested: (1) on the basis of the direction of attention, and (2) on the basis of proximity to the blind-spot.

If the observer had consistently attended to only one spot, the disap-

TABLE 1

O	Eye	Series I *Diff.*	Series I *S.D.* *diff.*	Series I Chances in 1000	Series II *Diff.*	Series II *S.D.* *diff.*	Series II Chances in 1000
H	Rt.	2.54	0.30	999.9	2.04	0.52	999.9
	Lf.	1.06	0.34	999.1	3.02	0.48	999.9
L	Rt.	0.95	1.00	829.0	3.27	2.21	930.9
	Lf.*	0.88	1.26	758.6	0.20	1.85	533.5
S	Rt.	9.38	2.72	999.9	9.42	1.56	999.9
	Lf.	5.35	2.52	983.1	4.26	2.14	978.9
G	Rt.	—0.14	0.62	589.0	—0.82	0.89	821.2
	Lf.	—0.71	0.45	942.9	—0.43	0.82	699.9

*The data for the left side of the left eye of Observer L in Series I included one value which was nearly nine *S.D.*'s farther from the mean than the value with the next highest deviation. On the basis of Irwin's (4) tables for the rejection of outlying observations this value was thrown out.

TABLE 2

O	Eye	Series III *Diff.*	Series III *S.D.* *diff.*	Series III Chances in 1000	Series IV *Diff.*	Series IV *S.D.* *diff.*	Series IV Chances in 1000
L	Rt.	1.77	2.07	803.3			
	Lf.	1.64	1.52	859.9			
S	Rt.	2.10	3.43	750.2	1.88	4.44	662.8
	Lf.	3.03	3.62	798.1	—3.23	4.42	767.9

TABLE 3

O	Eye	Series I	Series II	Series III	Series VI
H	Rt.	999.9	999.9		
	Lf.	999.1	999.9		
L	Rt.	829.0	930.9	803.3	
	Lf.	758.6	533.5	859.9	
S	Rt.	999.9	999.9	750.2	662.8
	Lf.	983.1	978.9	798.1	(—)767.9
G	Rt.	(—)589.0*	(—)821.2		
	Lf.	(—)942.9	(—)699.9		

*The signs refer to the direction of the differences to which the reliabilities pertain.

pearance of the spot to which he was attending might have been consistently reported first for that reason. Three considerations make the adequacy of this explanation improbable:

1) When, as in Dunlap's experiment and in our preliminary series, each observation was made with only one spot in the field, the same phenomenon occurred and with the same direction of difference as in the results reported above.

2) The time necessary to shift the attention and report the disappearance of the second spot would probably be of the order of twice a simple reaction-time, certainly less than ¼ second. The averages of the differences reported above for Series I and II are: S, 7.10; H, 2.66; L, 1.32; and G, —.52. It is improbable that a shift of attention only could determine differences of such magnitude. The shift of attention would have been a relatively larger factor in the case showing the smallest absolute difference than in any other case. The case showing the smallest absolute difference is the one which is negative in direction (observer G). Therefore, direction of attention may have been a factor in determining the inconsistency between G's results and those of the other *O*'s.

3) The most practiced observer, H, voluntarily reported that, although she was attending to one light, she still found it possible to report the earlier disappearance of the other light.

With regard to the blind spot, it has been suggested that proximity to this area might involve differential adaptation times. According to Wundt (9, p. 537), the blind spot subtends a visual angle of approximately 6° and its center lies at approximately 15° from the center of the fovea. The light spot used in this experiment subtended an angle of 18′, with its center at 10° from the center of the fovea. Therefore, the distance between the edge of the blind spot and the edge of the light spot was well over 1°. The light spot was probably well without the area of the blind spot, and the only question is whether or not the blind spot is surrounded by a region of modified receptivity. The fact that Dunlap found the same phenomenon at angles as large as 45° indicates that proximity to the blind spot is not the determining factor. The larger angles were not attempted in this experiment because of the difficulties attendant upon the use of the artificial pupil.

If adaptation time is correlated with sensitivity, as might be expected, then the side of the retina whose adaptation time is the longer would be the more sensitive. For five individuals, Dunlap, and four of our *O*'s, i.e., C (in the preliminary series), and H, L, and S, the temporal side has a longer adaptation time than the nasal for each eye. For observer G the nasal side shows a longer adaptation time than the temporal. The temporal side might, therefore, be expected to be the more sensitive for the majority of observers. Differential sensitivity of the two sides of the retina has been studied by Kirschmann (5), von Kries (6), Schön (8),

and Guillery (3) for angles ranging from less than 4° to 45° on each side of the fovea; all found the nasal side to be more sensitive, which is contrary to the inference above. Poffenberger (7, p. 50) reports a shorter reaction-time for the nasal than for the temporal side of the retina for several different angles. Presuming speed of reaction to be positively correlated with sensitivity, this would appear to corroborate the results of the other investigators.

That this contradiction is merely apparent is indicated by the following considerations: When the light spot first appears in the field its outline is sharply defined. Almost immediately a light haze appears in the surrounding area; after a few seconds the spot loses its sharp definition and is no longer distinguishable from the haze in form or brightness. If the observation is continued long enough, the haze gradually disappears and the field becomes uniformly dark. The spot was reported extinguished by all *O*'s when it was no longer distinguishable from the haze. The adaptation process may thus be divided into two phases: first, the gradual disappearance of the spot, which was the phase reported on, and secondly, the gradual disappearance of all brightness in the field. These two phases in the adaptation process suggest that two types of receptor mechanism are involved—the rods and cones of the retina. If this inference is correct, the intensity of our light must be somewhat above the cone threshold, which is shown to be the case by the fact that it is easily visible to the rod-free fovea. Hence there is good reason to think that the observations in the present experiment were made on a phenomenon of cone vision and are not in conflict with the results of investigations on threshold sensitivity, since in extra-foveal regions sensitivity for minimal stimuli is determined by the rods.

In view of the above results, the possibility is suggested that the functional distribution of the cones is different from that of the rods with respect to the two sides of the retina.

References

1. Crook, M. N. Central modification of visual sensitivity. (Master's thesis.) Worcester, Mass.: Clark Univ. Library, 1928.
2. Dunlap, K. Light spot adaptation. *Amer. J. Physiol.,* 1921, **55**, 201.
3. Guillery, —. Vergleichende Untersuchungen über Raum-, Licht-, und Farbensinn in Zentrum und Peripherie der Netzhaut. *Zsch. f. Psychol.,* 1896, **12**, 243-274.
4. Irwin, J. O. On a criterion for the rejection of outlying observations. *Biometrika,* 1925, **17**, 238-250.
5. Kirschmann, —. Ueber die Helligkeitsempfindung im indirekten Sehen. *Phil. Stud.,* 1888-1889, **5**, 447-497.
6. Kries, J. v. Ueber die absolute Empfindlichkeit der verschiedenen Netzhautteile im dunkeladaptierten Auge. *Zsch. f. Psychol.,* 1897, **15**, 327-351.

7. Poffenberger, A. T. Reaction time to retinal stimulation, with special reference to time lost in conduction through nerve centres. *Arch. Psychol.*, 1911-1912, **3**. Pp. iii+73.

8. Schön, —. *Arch. f. Ophthal.*, 1876, **22** (Abt. 4), **49**; (Abt. 24) Part I, 1878, **27**.

9. Wundt, W. Grundzüge der physiologischen Psychologie. (6th ed.) Vol. . Leipzig: Engelmann, 1911.

Wellesley College
Wellesley, Massachusetts

Clark University
Worcester, Massachusetts

A TEST OF THE CENTRAL FACTOR IN VISUAL ADAPTATION

Mason N. Crook

Introduction

The purpose of this experiment was to test, by the use of a somewhat modified procedure, the evidence for central inhibition or central fatigue reported by Dunlap (1) in a study of light spot adaptation and by Guilford (2) in a study of fluctuation of attention. The phenomenon is observed as the diminished sensitivity of a point on one retina for a light stimulus when the corresponding point on the other retina has been just previously exposed to the same stimulus.

In Dunlap's experiment the light was found to be invisible to either eye after sufficiently long pre-exposure to one eye. In Guilford's experiment a light was used of such intensity that, under continuous exposure, periods of visibility alternated with periods of invisibility, and inhibition was observed by shifting the light to the second eye during the invisibility period, measuring the time necessary for reappearance, and comparing this value with a value obtained in quite the same way except that the light was turned off for an interval during the invisibility period.

In the present experiment an effort was made to control certain variables in the experimental situation by using artificial pupils, by eliminating differences in exposure and recovery time in the observations to be compared, and by measuring the time from initial exposure of the light until first disappearance, instead of the time from first disappearance until first reappearance as Guilford did, thus eliminating one report by the observer.

Apparatus

The stimulus consisted of an achromatic light spot 1 cm. in diameter, having an illuminating value of 0.016 c. p., at a distance of 150 cm. from

the eye-piece and at a visual angle of 10° from the fixation point. The fixation point was the intersection of the two arms of a small, faintly luminous, red cross, at 150 cm. from the eye-piece. Fixation point and stimulus light were on the eye level. An artificial pupil 2 mm. in diameter was provided for each eye in all series but one. A shutter was used to expose the light spot to either eye at the will of the experimenter, but the fixation point was visible to both eyes at all times. Time was taken by means of a clock which was started by an electromagnet operated by the same switch which turned on the stimulus light. The clock was automatically stopped when the observer pressed a finger key upon the disappearance of the light spot.

Observers

Three observers were used—Dr. J. P. Nafe (N), Professor of Psychology; Wayne Dennis (D), graduate student in psychology; and the writer (C).

Procedure

All observers had sufficient training to become familiar with the stimulus light and to obtain some practice in fixation before any of the data here reported were obtained. Fifteen minutes were allowed for dark adaptation before each experimental period.

Observers were instructed to keep fixation on the center of the cross and report by means of the finger key when the light spot disappeared. A criterion of disappearance was adopted by all observers such that the disappearance was reported when the spot could no longer be differentiated from the surrounding haze, even though the haze might be somewhat brighter than the remainder of the field.

Four series of observations were carried out.

In Series I the light was exposed through artificial pupils to the two eyes alternately, one exposure every 30 seconds. In each exposure the light was kept on until the observer reported its disappearance, which usually occurred in 10 seconds or less. The light was then turned off for the remainder of the 30-second interval before exposure to the other eye. Thus each eye was stimulated for a few seconds once every minute, and 15 to 20 seconds of darkness intervened before each new exposure. The time until first disappearance was recorded for each exposure. In other words, Series I gave the average time for disappearance of the spot on each eye separately when the exposures for which the time was taken were preceded by intervals of darkness.

In Series II, also with artificial pupils, the light was exposed to one eye continuously for 30 seconds, then shifted to the corresponding point

of the other eye by means of the shutter. The observer reported upon the first disappearance after the shift. The light was then turned off, and, 30 seconds after the shift, it was re-exposed to the eye to which it had been exposed last. After 30 seconds of exposure the light was shifted back and the observer reported upon the first disappearance as before. From 10 to 25 such observations were made in each experimental period. Thus the two eyes were exposed alternately for periods of 30 seconds, and at the end of each 30 seconds' exposure the light was shifted to the other eye for the observer's report. Each eye was used for pre-exposure and each eye was used for comparison once every 2 minutes. Time was recorded from the shift until the first disappearance after the shift. Series II gave the average time for disappearance of the spot on each eye separately when the exposures for which the time was taken were preceded by 30 seconds of exposure of the light to the other eye. Results of this series were compared with those of Series I.

Possibly Series I and II are not comparable because of the different effect upon sensitivity of the different adaptation and recovery periods in the two procedures. In Series I an eye was rarely exposed for more than 10 or 15 seconds and was unexposed each time for about 50 seconds. In Series II an eye was exposed for 30 seconds, was unexposed for a full minute, was exposed again for perhaps 10 seconds, and was unexposed for about 20 seconds.

In Series III, also with artificial pupils, the light was exposed to one eye for 90 seconds, then shifted to the other eye for 90 seconds, then turned off for 90 seconds. The same succession of stimulating conditions was repeated through half the series, always the same eye being exposed first after the 90 seconds of darkness. During the other half of the series the succession was the same except that the other eye was the first exposed after darkness. Thus each eye was alternately exposed for 90 seconds and unexposed for 180 seconds throughout each half the series. Time was recorded from the beginning of each exposure until the first disappearance and also from each shift until the first disappearance. Series III gave the average time for disappearance of the spot on each eye separately both when the exposures for which the time was taken were preceded by 90 seconds of darkness and when the exposures for which the time was taken were preceded by 90 seconds of exposure of the light to the other eye. This made possible the same kind of comparison as between the results of Series I and II.

In all the above series artificial pupils were used. It was thought advisable, however, to take some observations with natural pupils to determine whether our experimental situation was comparable with those of other investigators, and whether removal of the control of pupil

fluctuations would cause any difference in the results under our conditions.

In Series IV the procedure was like that in Series II above except for three particulars: (1) Natural pupils instead of artificial pupils were used, with an intensity of stimulus which gave a brightness at the retina approximately equal to the brightness at the retina when artificial pupils were used; (2) the stimulus was presented every 3 minutes instead of every 2 minutes, 2 minutes being allowed for pre-exposure and nearly a minute for recovery; (3) the observer reported on the first disappearance in the 2-minute pre-exposure period as well as the first disappearance after the shift.

Results

The averages in Series I and II were computed for each eye of each of the three observers, and the difference between Series I and II for each eye was taken. By thus comparing the average time of disappearance when the stimulus light was presented to one eye immediately after it had been exposed to the other eye for 30 seconds, as in Series II, with the average time of disappearance when the light had not been previously exposed to the other eye, as in Series I, an index of the operation of central inhibition or central fatigue was obtained. Five of the six differences show that the light disappeared more quickly after it had been exposed to the other eye. For the right eye of C the difference is negative. All these differences are statistically reliable, including the negative one, though the ratio $Diff./S.D._{diff.}$ is smallest for the negative one. The means, the numbers of observations from which they are taken, the differences, and the reliabilities of the differences are listed in Table 1.

Nothing was observed in the experimental situation to account for the

TABLE 1

		D		C		N	
		Left	Right	Left	Right	Left	Right
Series I	N	80	79	40	40	39	40
	M	3.03	3.74	4.65	6.36	10.2	14.6
Series II	N	30	24	23	22	17	15
	M	1.38	1.47	3.86	8.26	5.7	9.13
	Diff.	1.65	1.27	0.79	—1.9	4.52	5.48
	$S.D._{diff.}$	.267	.264	.27	.61	.78	1.21
	$Diff./S.D._{diff.}$	6.03	4.76	3.33	3.12	5.79	4.53
	P.C.	999	999	999	998	999	999

conspicuous disagreement of the one negative value with the other values.

The averages in the two halves of Series III were computed for each eye of each of the three observers, and the difference between the two halves for each eye was taken. Thus a set of differences, uninfluenced by variations in adaptation and recovery time, but otherwise comparable to the differences between Series I and II, was obtained. The means, numbers of observations from which they are taken, the differences, and the reliabilities of the differences are listed in Table 2. Data were obtained from two observers only. The columns marked with asterisks form a set of observations in which all the periods were 60 seconds instead of 90 seconds.

The six differences all show that the light went out sooner after it had been just previously exposed to the other eye than after an interval of darkness. Four of the differences have very high, and two very low, reliability. The results are very similar to those in Table 1. The ratios $Diff./S.D._{diff.}$ in Table 1 are higher than those in Table 2. This is the only indication which the data show that the difference of the exposure periods in Series I and II might have had an influence on the results.

TABLE 2

		C Left	C Right	C Left*	C Right*	D Left	D Right
With pre-exposure	N	20	20	10	10	20	21
	M	5.53	10.6	4.37	8.3	2.39	4.3
Without pre-exposure	N	24	21	10	11	21	21
	M	6.4	13.9	6.87	8.64	3.55	4.82
	Diff.	1.05	3.35	2.5	0.34	1.14	0.52
	$S.D._{diff.}$	.36	1.18	.54	.41	.59	.6
	$Diff./S.D._{diff.}$	2.95	2.84	4.63	0.83	2.94	0.86
	P.C.	996	995	999	593	996	610

The average time for disappearance of the light when preceded by 50 seconds of darkness, and the average time when preceded by 2 minutes of exposure to the other eye, in Series IV, were computed for each eye separately, and the difference between the two conditions for each eye was taken. For each eye the time was shorter after exposure to the other eye, and the differences are highly reliable. The figures for the only observer used in this series are presented in Table 3.

TABLE 3

		C	
		Left	Right
With pre-exposure	N	6	6
	M	3.71	5.17
Without pre-exposure	N	6	5
	M	6.5	8.05
	Diff.	2.79	2.88
	$S.D._{diff.}$	0.68	1.19
	$Diff./S.D._{diff.}$	4.12	2.42
	P.C.	999	994

Summary of Results

Each positive difference in the above tables shows the influence of central inhibition, central fatigue, or some equivalent factor. The three tables together contain 14 differences, 13 of which are positive. Of the 13 positive differences, 11 show high reliability. Control of pupil fluctuations and of variability of adaptation time between series to be compared does not impair the reliability of the differences. On the whole, the reliabilities of the differences are comparable with those reported by Guilford. Thus our results are in accord with those of Dunlap and Guilford in showing that there is a real decrease in the sensitivity of a point on one retina when the corresponding point on the other retina has been just previously stimulated.

References

1. Dunlap, K. Light spot adaptation. *Amer. J. Physiol.*, 1921, **55**, 206-209.
2. Guilford, J. P. Fluctuations of attention with weak visual stimuli. *Amer. J. Psychol.*, 1927, **38**, 565-577.

Clark University
Worcester, Massachusetts

THE EFFECT OF GRAVITY ON THE OSCILLATIONS IN THE PATH OF AN ANT (*LASIUS FLAVUS NEARTICUS* WHEELER): A STUDY OF RANDOM MOVEMENTS

T. Cunliffe Barnes

The tropistic conception of animal movements, especially geotropism, is now well established (4); a representative range of organisms has yielded genuinely quantitative results, and, what is far more important, these

phenomena have been construed in the symbols of a rational theory.[1] Other phases of activity, however, are badly in need of investigation: the problem of "spontaneous activity" has been the subject of a searching inquiry (cf. 6) while our knowledge concerning "random movements" is extraordinarily inadequate. The evidence brought forward in this paper suggests that the methods of tropistic analysis may be extended with profit to the vexed question of "aimless" movements. The classical phenomenon of the tortuous path of the ant was found to be especially amenable to experimental control and we are again reminded of Wheeler's (13) remark: "There is no better group for the study of instinct, both as stereotyped hereditary activity and in its correlation with structure than the ants" (p. 522).

Many writers on myrmecology have alluded to the serpentine trails of ants. Lubbock (7) observed *Lasius niger* carrying larvae on a horizontal surface; the lines were relatively straight when the ant was "familiar with the road," while in others "the zig zag twistings show how much difficulty the ant experienced in finding her way." Bethe (1), working with the same species, showed that deviations in the path between the nest and a source of food were gradually eliminated by frequent passage.[2]

Cornetz (3) has written extensively on these oscillations and he observed that the turning was exaggerated when the ground was heated by the sun (3, p. 106) (*Tapinoma*). Cornetz was chiefly interested in the "homing" behavior of ants and believed that the oscillations in the path gave rise to "une mneme de direction."[3] The animal maintains an equilibrium in the side to side turnings and this activity leaves a trace in the nervous system which is utilized in the "homing."

Flying in sinuous lines and circles has often been described in the winged Hymenoptera (14), but the phenomenon is quite different from the changes in direction exhibited by a crawling animal. The work of Minnich (8) contains valuable measurements of the length of path and degrees of turning exhibited by a crawling hive bee, but only as influenced by phototropism.

---

[1]A feeble opposition to the tropism doctrine is still encountered in some quarters. Thus Hempelmann (5) objects that this philosophy renders the organism "viel zu einfach." Quite so! The tropism theory permits one to elicit the simple expression of a single type of response.

[2]Szymanski (12) refers such instances of the "dropping of useless movements" to the so-called principle of the shortest path. It will be recalled that Hertz attempted to make this principle the supreme law in physics (see 10).

[3]Cornetz (3) obtained evidence to support this curious hypothesis from Sagnac, who pointed out that oscillations increase the sensitivity "de certains régulateurs." This naïve and uncritical acceptance of notions from physics is far more grotesque logically than the most rabid vitalism (3, p. 107).

Lasius flavus was especially selected for the study of path oscillations because the regularity of these movements is particularly striking in this species and its minute size permits one to obtain relatively long trails for measurement. The ant was lifted gently on a bit of paper to the center of an inclined plane and the trail was followed on a sheet of paper with a soft pencil. The experiments were performed in the light, for the insect is rather difficult to see, and no change in behavior was produced by moving

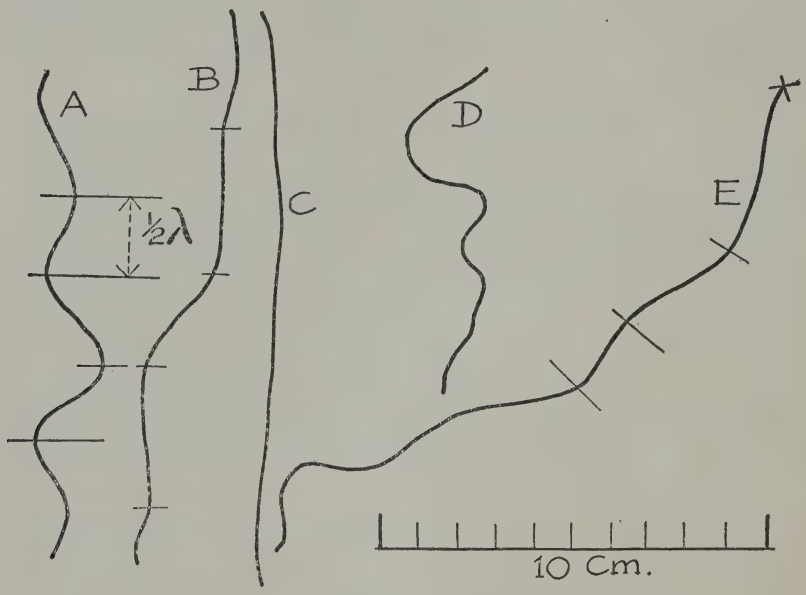

FIGURE 1

Tracings of actual paths obtained by following a moving ant with a soft pencil. *A, B, C* and *E* represent positive geotropic paths of *Lasius flavus; D* is a negatively geotropic path of *Aphaenogaster fulva.*

A, $\alpha = 40$, illustrating the wide amplitude of the oscillations at a low inclination of the surface and the method of measuring the "wave-length" $1/2\ \lambda$.

B and *C,* the same ant at $\alpha = 70°$ and at $\alpha = 80°$.

D, Aphaenogaster with antennae removed, $\alpha = 65$. The antennae-less ant exhibits wider oscillations of path.

E, Lasius flavus, $\alpha = 30$. The ant was made to run rapidly by touching it at X with the pencil; the oscillations are "smoothed out" although the inclination is less than for trail A. The "Θ" angle, however, is low.

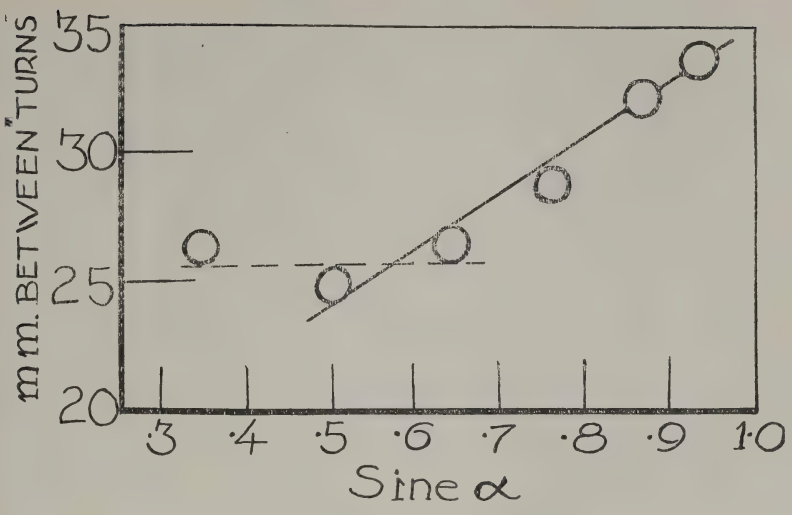

FIGURE 2

The length of path (as measured, mm., with a map tracer) between changes in direction of the path (or "turns") is plotted against the sine of the angle of inclination of the substratum (α). From $\alpha = 40$ upwards, these lengths of trail are a function of the gravitational pull.

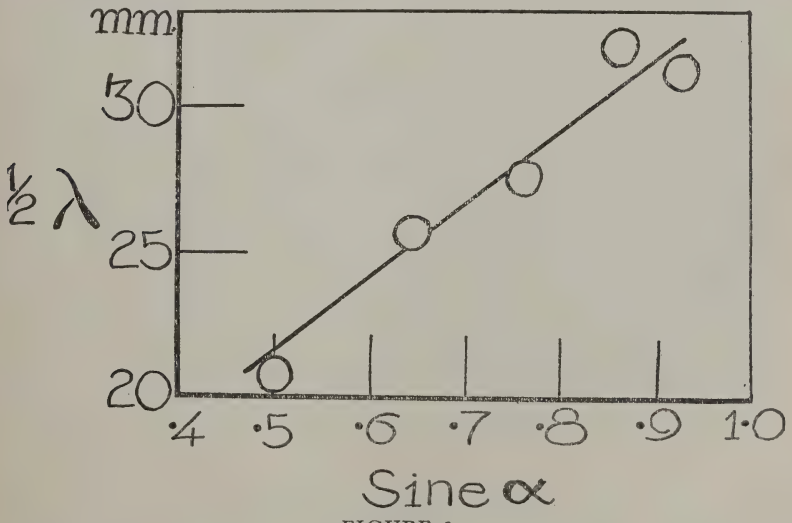

FIGURE 3

The relation between the wave-length (1/2 λ) of the oscillations in the path of a moving ant and the sine of the angle of inclination of the plane (*vide* Figure 1, *A* for the method of measuring 1/2 λ).

the source of light or by using a dim red light.[4] *Lasius flavus* was found to be positively geotropic and crawls down the incline in a sinuous path (Figure 1, *A*). As we have previously reported in *Crematogaster,* the amplitude of the turnings decreases when the gravitational component increases: at 80° inclination of the substratum the path is quite straight (Figure 1, *C*). In *Crematogaster* the sinuosity of the path is less marked, and it seemed permissible with that species to take short sections of the trail for the Θ angle, or angle of orientation as deviations from a line at right angles to the longitudinal axis of the plane; its rectilinear relation was found between the mean of 40 such measurements at each inclination and the orienting component of gravity. With *Lasius flavus* however, we were interested chiefly in the periodic changes in direction which form the waves in the path, and accordingly the wave lengths of the trail at various inclinations were measured. A rectilinear relation was found with the sine of the angle of tilt of the plane (Figure 3). The symbol $\frac{1}{2}\lambda$ represents the mean of 12 measurements of the distance from one peak in the wave to the next, as indicated in Figure 1, *A*. It would appear that three distinct phenomena are involved: first, the angle of geotropic response (Θ); secondly, the periodic change in direction to right and left; and thirdly, the degrees of turning away from the central direction. From the rational point of view it is quite gratifying to find all three factors exhibit a precise relationship with the gravitational vector and that there are no movements which would warrant the use of the epithet "random." The length of the path in millimetres between turns is plotted against sine α in Figure 2, where it will be seen that at high inclinations of the substratum the ant moves a greater distance before changing its direction (Table 2). The measurements were made by following the sinuous trail (about 500 mm. for each point on the curve) with a map tracer, and this length was divided by the number of times the ant had turned from side to side on that particular path. This fruitful method was adopted on the suggestion of Professor Crozier. It is obvious that Figure 2 represents the increase in the *speed* of creeping, which a priori should increase with the gravitational component.

Cole (2) attempted to show that the speed of movement in *Helix* under gravitational stimulation varies as sine α after orientation has been obtained. Pincus (9) established this notion in a more logical way with young rats by making proper allowance for the Θ angle. A low Θ and a high λ may be exhibited simultaneously in *Lasius* when the insect is made to run rapidly on the plane with low inclination (Figure 1, *E*). At high inclinations of the substratum the weight of the insect produces a

---

[4]Of course, it is well known that ants are sensitive to light under certain conditions (7, 11), but we hold that the light effects in our experiments were quite negligible under strong gravitational stimulations.

TABLE 1

THE "WAVE-LENGTHS" OF THE OSCILLATIONS IN THE PATH OF *Lasius flavus*, $\frac{1}{2}\lambda$ (VIDE FIGURE 1, *A*) AT EACH OF SEVERAL INCLINATIONS OF THE SUBSTRATUM (α)

(Each "$\frac{1}{2}\lambda$" represents the average of 12 determinations.)

α (degrees)	$\frac{1}{2}\lambda$ (millimeters)	*P.E.* (millimeters)
20	23.5	$\pm$1.11
30	20.5	$\pm$0.86
40	25.4	$\pm$1.44
50	27.5	$\pm$0.92
60	32.0	$\pm$2.16

TABLE 2

THE LENGTH OF PATH IN MILLIMETERS BETWEEN "TURNS" (VIDE FIGURE 1, *B*) AS MEASURED BY A MAP TRACER

Each value represents about 500 mm. of path divided by the number of turns. *Lasius flavus* measures about 2.5 mm. long.

α (degrees)	Distance between turns (millimeters)
20	26.6
30	25.0
40	26.6
50	28.8
60	32.2
70	33.7

tension on the upper part of the supporting musculature, and the resulting proprioceptive effects lead to adjustments in the activities of the legs until the difference in the pull of the body weight on the two sides is reduced to a threshold value. The side-to-side circling, which may completely reverse the animal on a level surface, is thus checked by the gravitational component. A record of the measurements is given in Table 1. It will be noted that the highest probable error was found in the $\frac{1}{2}\lambda$ values at a high inclination, where the smoothing out of the path varies in each wave.

It would be quite beside the point to discuss the possible significance of these path oscillations (Turner's curves) in the "homing" of ants. Our results are to be considered as an attempt to study the "random movements" of insects which still await rational analysis.

SUMMARY

The random movements exhibited by a crawling ant on an inclined surface are a function of the gravitational component.

References

1. Bethe, A. Dürfen wir den Ameisen und Bienen psychische Qualitaten zuschreiben? *Arch. f. d. ges. Physiol.*, 1898, **70**, 15-100.
2. Cole, W. H. Geotropism and muscle tension in *helix*. *J. Gen. Physiol.*, 1925, **8**, 253-263.
3. Cornetz, V. Les explorations et les voyages des fourmis. Paris: Flammarion, 1914. Pp. 192.
4. Crozier, W. J. Tropisms. *J. Gen. Psychol.*, 1928, **1**, 213-238.
5. Hempelmann, H. Tierpsychologie. Leipzig: Akademische Verlagsgesellschaft, 1926. Pp. 676.
6. Hoagland, H. Quantitative aspects of tonic immobility in vertebrates. *Proc. Nat. Acad. Sci.*, 1927, **13**, 838-843.
7. Lubbock, J. Ants, bees and wasps. New York: Appleton, 1894. Pp. 448.
8. Minnich, D. E. The photic reactions of the honey-bee. *J. Exper. Zoöl.*, 1919, **29**, 343-425.
9. Pincus, G. Geotropic creeping of young rats. *J. Gen. Physiol.*, 1927, **10**, 525-532.
10. Planck, M. A survey of physics. London: Methuen, 1925. Pp. 189.
11. Santschi, F. Comment s'orient les fourmis. *Rev. suisse de Zoöl.*, 1913, **21**, 347-426.
12. Szymanski, J. S. Das Prinzik der kürzesten Bahn in der Lehre von der Handlung. *Biol. Zentbl.*, 1917, **37**, 282-287.
13. Wheeler, W. M. Ants. New York: Columbia Univ. Press, 1913. Pp. 663.
14. Wolf, E. Über das Heimkehrmögen der Bienen. *Zsch. f. vergl. Physiol.*, 1927, **6**, 221-254.

Harvard University
Cambridge, Massachusetts

ON THE HISTORY OF RESEARCH WITH RATS AND MAZES: A COLLECTION OF NOTES

Walter R. Miles

It is now several years since the rat was adopted as a laboratory animal, and maze studies were begun. The literature is already quite large and there are doubtless a number of individuals who are interested in the early history of this research field. In February, 1928, letters were addressed to Professor Linus W. Kline, Dean Willard S. Small, and Dr. Adolf Meyer requesting early data or memoranda in reference to the use of the white rat. These gentlemen very courteously replied and sent material. Later, similar requests were made of professors Colin C. Stewart, Irving Hardesty, C. F. Hodge, J. R. Slonaker, H. H. Donaldson, John B. Watson, and others. In most cases these requests resulted in replies containing discussion which seems worth preserving.

The first reply came from Professor Kline who, on February 18, 1928, informed us as follows:

"I am writing L. N. Wilson, Librarian at Clark, to check some of my references to old books at Clark containing wood cuts of various sorts of mazes and from which I made drawings of the first mazes used at Clark in 1898-1899."

Five weeks later, at the time of communicating his notes, he sent a prefatory letter which may be quoted in part:

Farmville, Virginia
March 27, 1928

My dear Professor Miles:–

I am sending you by parcel post the promised manuscript on the early history of the use of boxes and of mazes in studying white-rat-learning. This is my vacation and I am a bit late in getting the work to you.

I have had drawings made of the two boxes. These drawings are with the manuscript. The original drawings of box #1 appear on page 28 of my old note book which I have taken the liberty to send along. Figure (*c*) is added to make clear the exact structure of the inward swinging of the door. Figure (*b*) in the drawing of box #2 is also added, otherwise the drawings are similar to the pencil sketches made in 1898.

You will notice on pages 20-21 and on page 42 entries made in a new hand. This was done by Dr. W. S. Small and I merely mention it to show that we were good fellows together working in the same laboratory mutually sharing each others plans and ideas. But even so I fail to recall at this distance his use of my boxes but I know he did as described in his published reports. Nor do I recall how or when he decided to adapt the Hampton maze to his work but it must have been the later part of 1898.

I do not think Adolf Meyer had anything to do with the introduction of the use of white rats in the Psychol. Laboratory at Clark. Both Colin C. Stewart and H. F. Hodge and others had used these rats before Meyer came to America, and I know I got my idea of using white rats from observing Stewart's work. It is true I had conferences with Meyer but they related to other problems and the question of using white rats never arose. I trust I have cleared up the early study of the white rat at Clark and made it clear that the idea of the Hampton maze came from Professor Sanford. I have enjoyed going over this ancient history and trust that I have put it in shape to be used.

Very truly yours,
(Signed) Linus W. Kline

Kline's Early Psychological Work with Animals

In attempting to answer your inquiry about the early history of maze work with animals at Clark University, it may be in order to say that I entered Clark in 1896 with the intention of studying "zoological psychology." In shaping a problem for the doctorate thesis I was steered to a compromise and began work on "The Migratory Impulse *vs.* the Love of Home," believing that this problem offered legitimate grounds for experimenting on the effect of certain physical factors on the migration of animals.

During the fall and winter of 1896-97 I studied the effect of temperature on the movements of tadpoles, adopting the method used by Verworn in a similar study on paramaecium. Also with children I studied the effect of

hunger and bad social conditions on truancy. The other half of the problem, "The Love of Home," also called for experimental observation. Problems like the "homing" of pigeons and of the honey bee, of the persistence and directness of domestic animals in returning home, and the effect of confinement upon animal life naturally arose. The latter problem prompted a study, during the summer of 1897, on the effect of solitude on the rate of the growth of chicks (results unpublished). I had hoped to find methods that might be applied to the study of the dark problem of nostalgia, "sickness for home." It was during this work on chicks and while reading the chapter on "Association in Animals" in C. Lloyd Morgan's *Introduction to Comparative Psychology*, that the idea occurred of using little boxes somewhat as harmless traps to study the ways by which rats search for food in out-of-way places. The notion of using rats came from observing an extended investigation conducted by Colin C. Stewart in the Department of Biology at Clark, 1895-97. Stewart's methods, apparatus, and technique were both interesting and instructive and made a decided impression upon me. He justified the use of rats and mice on the grounds that "They are small, cheap, easily fed and cared for; and best of all, when placed in revolving cages they spend most of their time, when not eating or sleeping, in running." (See Colin C. Stewart: "Variations in Daily Activity Produced by Alcohol and by Changes in Barometric Pressure and Diet, etc." June 19, 1897, p. 41. Thesis for the Degree of Doctor of Philosophy, Department of Biology, Clark University.)

It was not until the fall of 1898, however, that opportunity occurred for beginning a psychological study of the formation of "association" in rats. In planning the first series of experiments, I was guided chiefly by two ideas: (1) that the rat is a "gnawer"! and (2) that the gnawing must be done under the hunger impulse, free from fear, and, as far as possible, under natural conditions. At that time I was impressed with the importance of working with animals in as natural moods as conditions permitted. For this purpose, mouse box No. 1 was constructed. This box, although *made first,* is described as No. 2 on page 426, by L. W. Kline, *Amer. J. Psychol.,* 1899, **10**, April, and is also referred to by W. S. Small in a footnote to the same journal vol. **11**, pp. 29-30. There occurred in the same footnote an abbreviated account of the experiments given herewith in Table 1. (The reader may be referred to Linus W. Kline, "Methods in Animal Psychology," *Amer. J. Psychol.,* 1898-99, **10**, 256-279, section on "The White Rat," pp. 277-279; and W. S. Small, "Suggestions Toward a Laboratory Course in Comparative Psychology," *Amer. J. Psychol.,* 1898-99, **10**, 399-430, section on "The White Rat," pp. 419-424 for other details. Ed.) This box was 7 inches square and 6 inches high; sides of wire mesh; top of glass; bottom of wood. The side view is here shown in Plate 1, Figure *a*; the entrance and front view in Figure *b*. The entrance is an opening 2½ inches square, provided with an inward swinging door of sheet zinc hinged at the top

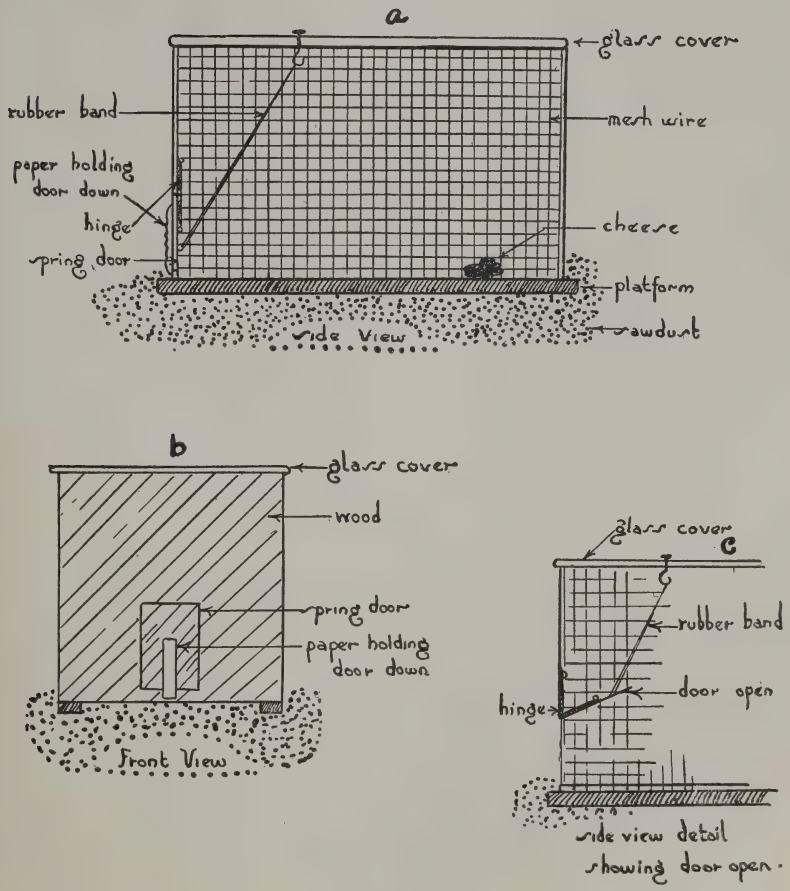

PLATE 1

THE DETAILS OF THE FIRST MOUSE BOX MADE BY KLINE IN 1898 AT CLARK UNIVERSITY

of the opening. The lower end of the door was caught by a rubber band stretched from a hook fastened in the glass top. Figure *c* shows this arrangement in detail.

While the first series of observations, seven in number, were cut short by the death of the animal, yet because they mark the first attempt to investigate the learning of the white rat they are here transcribed in tabular form as recorded at the time in my notes of 1898 which are still preserved.

TABLE 1

KLINE'S FIRST LEARNING EXPERIMENT ON THE WHITE RAT

No. of exper.	Date	Hour	Time for opening door —minutes	Remarks
1	Dec. 3, 1898	9:10 A.M.	10	Marked desire to get in box. Tore paper loose in 9 min.
2	Dec. 3, 1898	9:45 A.M.		Worked 20 min. and gave it up.
3	Dec. 3, 1898	3:50 P.M.	45	Opened door by butting with head.
4	Dec. 4, 1898	3:55 P.M.	5	Tore paper with mouth.
5	Dec. 4, 1898	4:03 P.M.	3	Localized place but did not recognize paper.
6	Dec. 4, 1898	4:08 P.M.	2½	Food taken from rat in 4th, 5th, and 6th experiments.
7	Dec. 4, 1898	4:17 P.M.	..	Rat appeared stolid or sick, refused to work.
..	Dec. 5, 1898	9:00 A.M.	..	Rat found dead.

This first series of observations developed the fact that the white rat was a "digger" as well as a "gnawer" and perhaps more so. To test this point, I made box No. 2 (see Plate 2) described in *Amer. J. Psychol.*, **10**, p. 277. The box was 8 inches long, 7 inches wide, 6 inches deep; sides of wire, top of glass, bottom of wood. Plate 2, Figure *a* shows the box raised above the level of the floor by resting on two strips 1½ inches thick. Figure *b* of the same box shows a bottom view with a piece 3½" x 2" sawed out at one end. This small experimental box, like box No. 1, was placed in a large home box 18 inches long, 14 inches wide and 14 inches deep; one side was wire, one end glass, the rest wood. The rats to be used in the experiment knew practically no other home as they had been reared in much the same sort of box. They were then placed in the home box along with box No. 2 several days before the time of the experiment in order to become familiar with their surroundings. A series of 13 observations using this box were made on two rats beginning January 9, 1899, and ending January 23. The first five observations are described in *Amer. J. Psychol.*, **10**, p. 277 and are also referred to by Small in the same journal, **11**, pp. 25-26.

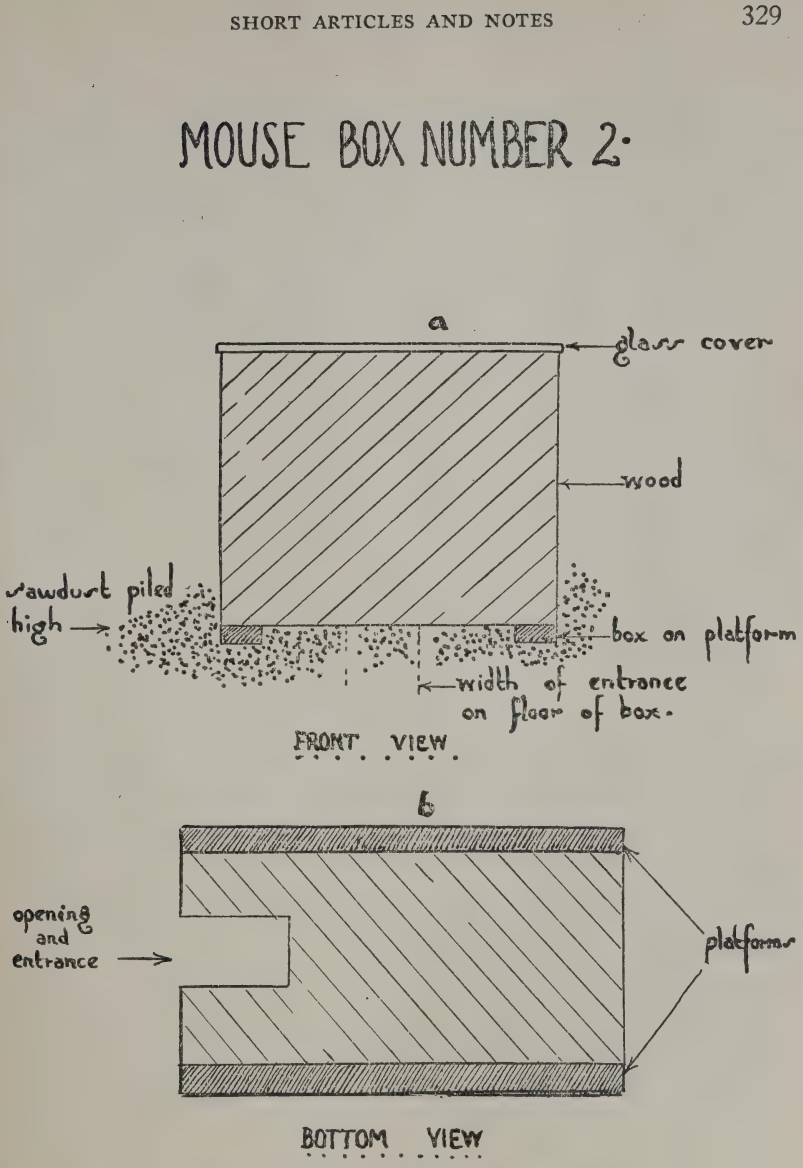

PLATE 2

ARRANGEMENT OF MOUSE BOX USED BY KLINE IN 1899 TO STUDY THE DIGGING AND GNAWING OF RATS

TABLE 2

A Series of Experiments in Which Two Rats Were Jointly Engaged in Securing Food from Box No. 2

No. of exper.	Date	Hour	Time to dig into box
1	Jan. 9, 1899	2:00 P.M.	1 hour, 30 minutes
2	Jan. 10, 1899	3:45 P.M.	8 minutes
3	Jan. 11, 1899	2:12 P.M.	2 minutes, 30 seconds
4	Jan. 12, 1899	4:17 P.M.	3 minutes
5	Jan. 13, 1899	4:15 P.M.	3 minutes, 30 seconds
6	Jan. 14, 1899	4:30 P.M.	2 minutes, 30 seconds (observation incomplete)
7	Jan. 16, 1899	5:30 P.M.	1 minute
8	Jan. 17, 1899	5:30 P.M.	5 minutes (rats disturbed by noise in room)
9	Jan. 18, 1899	4:30 P.M.	1 minute, 30 seconds
10	Jan. 19, 1899	5:30 P.M.	1 minute, 45 seconds
11	Jan. 20, 1899	2:30 P.M.	1 minute, 30 seconds
12	Jan. 21, 1899	5:30 P.M.	30 seconds
13	Jan. 22, 1899	5:00 P.M.	45 seconds

The complete record taken from my notes is given here in Table 2 for the first time.

The complete protocol for the experiment, January 9, 1899, is as follows: "Food (dog biscuit and cheese) was placed in the box at 2:00 P.M. The rats attacked the box at once, crawled up its side, over its top and round and round until it grew quite monotonous watching them. At 3:00 P.M. their activities abated—their movements less quick and more haphazard, one began playing at the side of the box where the piece had been sawed out. The hole scratched was about the size of the head, it immediately poked its head in the excavated place and seemed to be frightened; it ran away to a hiding place. Rat came out in two minutes, smelled around the hole and dug at some sawdust, then ran away as before. This was repeated several times until a hole quite too large had been excavated. It then ventured cautiously up into the box, snatched the food and made way with it at 3:30 P.M." The total time for the two rats to solve the problem was therefore 1 hour, 30 minutes. (It is not stated whether both rats actually engaged in the digging. Ed.) Protocol for January 10, 1899, 3:45 P.M. "The two rats behaved much like on the preceding day except they spent more time about the place where they had excavated the day before—as if they located the place, although this seemed very indefinite. They walked and fidgeted about for 4 minutes then one began digging away the sand and sawdust and at the end of the fifth minute the work was completed. As before they hesitated to go straight in, they frisked about nervously for another minute and at about 3:53 P.M. or after 8 minutes work they secured their dinner." January 16, 5:30 P.M. "Came out at sight of

box—went at once to work and in *one minute* had secured the food, the quickest record yet made."

The 13th experiment was the last made in this series owing to other pressing work, but I felt that the rat had not reached its maximum speed which at that stage of learning appeared to depend upon *intensity* of hunger and upon absence of distraction. The former surmise has since been investigated by Professor Washburn. The two types of boxes figured were later used more extensively by W. S. Small.

The idea of using a maze for studying the "home-finding" capacities of the rat came from Professor Edmund C. Sanford during a conversation I had with him about the burrowing and digging activities of the rat. This conversation occurred in the spring of 1898 before I had finished work on my doctorate thesis. I had described to him runways which I had observed several years ago made by large feral rats to their nests under the porch of an old cabin on my father's farm in Virginia. These runways were from three to six inches below the surface of the ground and when exposed during excavation presented a veritable maze. Sanford at once suggested the possibility of using the pattern of the Hampton Court maze for purposes of constructing a "home-finding" apparatus. That was my first acquaintance with mazes. I looked up the Hampton Court pattern and made a copy of it, but from what source I am now unable to determine, perhaps it was from the 9th edition of *Encyclopedia Britannica.* It is not claimed that my account of the burrowing rats was an initial suggestion to Professor Sanford. In fact the readiness with which he directed my attention to the Hampton maze is presumptive evidence that he had thought of its use before. I should say that my interest in the maze at the time related to its fitness to study the "home-finding" or "food-box–finding" capacity of animals and not as an apparatus for the study of animal learning as it was used later by Small. My interests and experiments with rats in box-learning as above described came later. Other work prevented the use of the maze in my problem and prevented my making any further study in that direction.

As I recall, my attention was again directed to the Hampton maze in January, 1899, by observing W. S. Small constructing his first maze in the hallway in front of an entrance to the laboratory machine shop of Clark University. Small now began extended series of experiments on maze learning with the white rat which not only marked the first use of the maze in the study of animals, and determined its experimental value but also by his published reports gave to comparative psychology a model for painstaking, judicial procedure and for conservative interpretation of results.

(Signed) Linus W. Kline

Skidmore College
March 27, 1928

Small's Account of Early Work With Mazes and Rats[1]

University of Maryland
College Park, Md.
March 29, 1928

My dear Professor Miles:

About the time I received your letter I received one from Dr. Warden asking for similar information for use in a book he is writing on the History of Comparative Psychology. I am sending you a copy of my letter to him, as it covers the information you seek. One question in your letter is not covered in my letter to Warden. The maze pattern I used was the Hampton Court pattern, corrected to rectangular form. No other maze pattern was tried.

I shall be very glad to have you make a "note" for the *Journal of General Psychology* if the material I am sending seems significant.

Yours very truly,
(Signed) W. S. Small, Dean

March 29, 1928

Professor C. J. Warden,
Department of Psychology,
Columbia University,
New York City, N. Y.

My dear Professor Warden:

I have finally got a line on a photograph and will send the print in a few days. It is quite recent.

I have been unable to find any contemporary notes, so I must depend upon my memory for the facts you desire. As nearly thirty years have elapsed—years in which Rat Psychology has been a gradually vanishing item in my mental furniture, I cannot be sure that my memory is exact.

I. So far as I know I did first make use of the albino rat as a psychological subject; and so far as I can recall, the work was begun in the spring of 1898. Three factors in the matter, I think I recall with reasonable certainty. (1) I had been working a little with Kline on insects—chiefly study of tropisms. (2) I had, that year, been carrying on with Dr. C. F. Hodge, a minor study on transmission of acquired characters using the albino rats for experimental purposes. I had kept a fairly large number and had become rather familiar with their "manners and customs" and had developed a liking for the animal. (3) At the same time, I had been making a preliminary study on the Development of Altruism and had dipped into Darwin, Morgan and others for possible material in the sub-human species. In particular, I read thoroughly Sutherland's "Origin and Growth of the Moral Instinct." (Reviewed it for the *American Journal of Psychology*).

These three factors tended to focus my interest upon a study in compara-

---

[1]Dr. Small's first reply was under date of February 23, 1928, when he said:

"Thirty years is a long time and I find that most of the specific items of fact that you ask about in your letter of February 10th are very hazy in my mind.

"It is possible that I have packed away somewhere the original notes in regard to the use of the maze in my old study of the rats. I shall have to do a little "house cleaning" to determine whether I still have those notes or whether they have been destroyed. In either event I will write you very soon as nearly as I can recall, the facts that you desire."

tive psychology as a probable thesis project. My familiarity with the albino rat was, I believe, what determined that animal as my special object of study.

II. I am not very clear as to the origin of the idea of using Hampton Court pattern as the main experimental device. As nearly as I can remember, the suggestion came from Dr. E. C. Sanford after I had done some of the preliminary work reported in my first paper. I was working under Dr. Sanford's direction. I recall that we had a number of consultations relative to methods of procedure and that I discussed with him the characteristic activities of the rat and the necessity of devising apparatus that would conform with those characteristic activities. I feel quite certain that Dr. Sanford responded with the maze suggestion. Memory is not clear on this point, but the "feeling" is so insistent that I have no doubt that the "guilt" was his.

III. As to the boxes used in my work, I cannot remember clearly but my impression is that Kline and I thought those out together, though it is possible—probably even—that Kline devised both of them. I shall be glad to have the credit go to him, as I know he was more resourceful in such matters than I was.

IV. My early work was in no way influenced by Thorndike as I did not know of his work when I made my approach to the problem. His study was published soon after I began my experimental work. I studied it carefully and thoroughly—and with profit. The chief advantage I derived from it was to make me more certain that I was on the right track in seeking to conform experimental procedure to the native tendencies of my animals and to sharpen criticism of my own methods and results.

V. I am unable to say "whence came the interest at Clark in Animal Psychology from the experimental standpoint"; but my judgment is that it had no specific *locus in quo*. Dr. Hall's insistent interest in evolutionary psychology was surely one element in the situation, but it may interest you to know that he did not give me, at first, much encouragement in my project. He doubted whether laboratory methods could be devised that would yield returns of much value. Dr. Sanford was just developing an interest at the time I began my work. My belief is that Kline was the instigator of Sanford's interest. He preceded me and I feel rather certain that he really opened up the matter to Sanford. At any rate it was Kline's work that first enlisted my interest and made clear to me the possibilities of laboratory study of animal psychology.

I hope this rather discursive narrative will enable you to pick out the facts you need. Of course, I shall be glad to give other items of information if you wish—and my memory is not too treacherous.

Yours very truly,

(Signed) W. S. Small, Dean

Meyer on the First Laboratory Rat Colonies

The Johns Hopkins Hospital
Baltimore, Md.
October 1, 1928

Dr. Walter R. Miles
Dept. of Psychology
Stanford University
California

My dear Dr. Miles:

The only person that remains to be asked is Colin Stewart of Dartmouth.

From all I have been able to reconstruct white rats must have been in use through Hodge and others at Clark and elsewhere.

My own share lies in the fact that I raised large numbers in Worcester and urged Donaldson to make the white rat the standard animal (May, 1897), and furnished him with a large lot, handled by Hardesty.

The use for maze work was evidently independent, and based largely on the use of these animals in Hodge's laboratory—and on good sense in choosing a reasonably *manageable* animal.

The Wistar Colony on the other hand had an impetus through Donaldson and it has done a good job.

Sincerely yours,

(Signed) Adolf Meyer

STEWART ON THE EARLY EXPERIMENTAL USE OF THE RAT

Dartmouth College
Hanover, N. H.
October 7, 1928

Dr. Walter R. Miles
Stanford University
My dear Dr. Miles:

I have been much interested in your letter and in one received from Dr. Meyer, with reference to the early work with white rats. My work at Clark University during the years 1894-1897 was undertaken at the suggestion of Dr. Hodge. At his suggestion the wild gray rat was used as the experimental animal. The difficulty of handling the gray rats (1894-95) was the reason for changing to albino rats for the remaining two years of my stay at Clark (1895-1897). No one was in any way associated with me in the animal activity work. There were no white rats in the department before 1895, and there were none but those employed in my experiments during the years 1895-1897. I do not recall receiving any white rats from Dr. Adolf Meyer, but it is entirely possible that I did as he came to the Worcester Hospital in 1895. Albino rats were on sale in bird and animal stores and I know that I purchased some in Worcester.

I should say that whatever credit there is in having suggested the use of *rats* as experimental animals, belongs to Dr. C. F. Hodge. If anyone wants to know why I changed from wild gray rats to *white rats* in 1895, let him work with gray rats for a year. I don't deserve any credit for the change. Whether I gave white rats to Dr. Meyer or he gave some to me I do not recall, but he alone was responsible for any that were sent to Prof. Donaldson at the Wistar Institute.

Prof. Kline's work with white rats was done, probably under Dr. Sanford, after I left Clark University in 1897, and probably with the lineal descendants of my "animal activity" stock.

Dr. A. M. Cleghorn used white rats and the same recording methods in Boston in 1897-98, in testing the effects of orchitic extracts on voluntary activity.

Yours sincerely,

(Signed) Colin C. Stewart

SLONAKER ON THE EARLY USE OF THE RAT

Stanford University, Calif.
January 15, 1929

Dear Dr. Miles:

I have read with much interest the data you have collected in regard to the early history of the use of the albino rat in animal experimentation. I will add some notes and suggestions which may be of assistance to you.

I entered Clark the fall of 1893 and entered at once into an investigation

of the comparative anatomy of the point of acute vision in vertebrates under the direction of Dr. C. F. Hodge. This I pursued continuously for the three years I was there, taking my degree the spring of 1896. During the first year I have no recollection of any rats or mice being used in any of the departments. Stewart entered the fall of 1894 and, working under Hodge, began his work with the gray rat as stated in his letter to you. During this year I made use of the gray rat in a fatigue experiment as a minor problem, but never published the results. This, however, was after Stewart had begun his work as I used one of his spare cages in my experiment. I feel confident that Stewart was the first to use the rat at Clark.

I would suggest that you communicate with Dr. H. H. Donaldson at Wistar Inst. and with Dr. Irving Hardesty, Tulane University, New Orleans. Donaldson had left Clark and gone to Univ. Chicago before I went to Clark. Hardesty was associated with him in Chicago and is a very dear friend of mine. If Meyer sent Donaldson some rats it must have been to Chicago for he was not in this country after until Donaldson had gone to Chicago. I was associated with Donaldson in Chicago during the years 1901-1903. At that time they had a well established colony which both Watson and myself made use of. The nucleus of this colony may have come from Meyer. Donaldson or Hardesty can inform you.

I do not know of any published account of the origin of the Wistar Colony. However, when I took lunch with Donaldson while in Philadelphia some three years ago, I asked him or Dr. King, if the original stock did not come from the Chicago colony and was informed that it did. Our colony here at Stanford also came from the same source.

If there is anything more I can give please let me know and I will comply.

Very sincerely,

(Signed) J. R. Slonaker

Hardesty on the Early Laboratory Adoption of the Rat

Tulane University
New Orleans
January 24, 1929

Dr. Walter R. Miles
Department of Psychology
Stanford University
Dear Prof. Miles:

In reply to your inquiry of Jan. 17, I am sorry to say that as to personal experience I know little of the use of the white rat prior to 1897. I am quite sure that in the latter part of this year Prof. Donaldson in Chicago, after deliberation, decided to try out the white rat with a view to replacing the frog as the general laboratory animal in his projected program for his laboratory, namely, problems dealing with the Embryology, Growth and Functional Development of the Nervous System. Though a few papers from his laboratory after this year used the frog, my thesis included, we were beginning to use the rat, and I remember that in 1898 one of our regular class laboratory exercises in his course on the growth of the nervous system was to measure the internodes of the freshly taken nerves of rats of different ages.

Dr. Donaldson's laboratory had a small supply of rats in 1896 and maybe prior to 1896. I think it was in 1898 that Dr. Adolf Meyer sent us two or three dozen adults and these were chiefly those from which the Donaldson colony started, later so elaborately developed into the Wistar Colony. I remember Donaldson advocating the white rat for general use both with the Chicago staff and with men from other laboratories in the year '96-'97.

He may have begun it earlier. I taught in the University of Missouri in '95-'96, having had to drop out for a year to make some "Pin money." I had thought all along that the idea originated with Donaldson till learning from Dr. Meyer's statement that he had urged Donaldson to make the rat the standard animal. I have no reason, however, to doubt Dr. Meyer's statement.

The first use of the white rat as a maze animal that I know of was in the work of Slonaker and then Watson. I did not know of Dr. Stewart's work till reading the copy of his letter you sent me.

I went to the Department of Anatomy of the University of California from Chicago in 1901 and have not been closely associated with the matter since. My memory may not be wholly trustworthy as to the dates. I wish I could be of more definite help. I am sure that the present general use of the rat is due more to Donaldson's advocacy and use of it in the papers done under him than to any other man.

Yours very truly,
(Signed) Irving Hardesty

DONALDSON ON THE HISTORICAL RAT PROBLEM

Wistar Institute of Anatomy
Philadelphia, Pa.
January 29, 1929

Dr. Walter R. Miles
Department of Psychology
Leland Stanford University
Stanford University, Calif.
Dear Dr. Miles:

Yours of the 17th, with enclosures, reached me duly and I regret the tardiness of my response. To do what I can I am sending this to you by air mail.

Your primary question relates to the use of the rat in maze studies. I have nothing to add to the information contained in the documents save to note that in his book published in 1903 bearing the title of "Animal Education", John B. Watson figures several mazes and refers to the work of Small. Watson's study was begun in the fall of 1901 and has an interest merely as the first study employing the maze made at the University of Chicago. It is, of course, several years later than the pioneer investigations.

As I see our historical rat problem there is the one in which you have an interest, then comes the first use of the rat in my laboratory as a general experimental animal and the employment of the rat for nutritional studies at Wisconsin and New Haven. There are, roughly, three separate topics and as you note by the correspondence I am taking up the one that relates to my laboratory at Chicago, with Dr. Meyer and others concerned.

I am much interested in this little investigation and hope you will let me know when it is put in final form.

Yours sincerely,
(Signed) Henry H. Donaldson

So far as we may judge from the foregoing letters the laboratory use of the white rat began in 1895. In that year we are informed by Professor Stewart that he secured and used albino rats in Hodge's laboratory at Clark University, following a previous experimental experience with the wild rat which had been undertaken at the suggestion of Professor Hodge. It

appears that Donaldson and perhaps also Meyer began work with the white rat at Chicago almost if not quite as early as did Hodge at Clark. The relationships of these two centers as regards the adoption of the animal are not perfectly clear. Probably Professor Hardesty's statement is correct that Donaldson's continuous work with the rat has done much to bring about its present general acceptance for laboratory use. We judge from the evidence submitted that Sanford was the first to conceive the idea of using the maze as an implement for studying the behavior of the rat and to bring about the completion of investigations of this character.

Stanford University
California

A GRAPHICAL METHOD FOR FITTING CURVES TO LEARNING DATA

WILLARD L. VALENTINE

In the process of fitting learning records with curves, the graphical method herein described was developed. It is open to all the objections that are raised to graphical method, but reduces the time required to fit a curve by the "least square method" from hours to minutes. It is adaptable to any kind of curve fitting where the form of the curve is known or can be approximated, and where it is desired to evaluate several parameters. The curve to be discussed was suggested by Meyer and Eppwright (1) for fitting learning records. An examination of its equation,

$$t = k \; arc \; cot \; (n + p) + t_l$$

where t is the time required to complete an act, including the correction of any errors that may have occurred, and n is the number of the practice period, as *0, 1, 2, 3, ...*, will show that three parameters k, p, and t_l must be evaluated. Curves may be plotted using n and $k \; arc \; cot \; n$ as co-ordinates as shown in Figure 2. Here t_l and p are both equal to zero. But as p is to be added to n its only influence on the curve is to shift the curve to the right or left depending upon whether p is minus or plus. In a similar manner, t_l is an addative constant which shifts the curve up from zero. It can never be negative. When one has to guess at the proper combination of parameters to fit the curve, enormous quantities of time are consumed.

The curves of Figure 1 were scratched into the surface of a piece of celluloid in the same scale that is used to plot the raw learning scores. The resulting crevices were stained with black waterproof drawing ink and then smoothed off with collodion. The number of

curves to be used will be determined by the accuracy with which it is desired to determine k and the limit of accuracy of the graphical method.

The raw data, plotted on ordinary graph paper, is attached to the ground glass top of a drawing or tracing table by means of commercial adhesive tabs made for that purpose. The paper is "lined-up" to a straight edge of a Universal Drafting Machine, the protractor of which appears in Figure 1. The straight-edge is then slipped out of its holder,

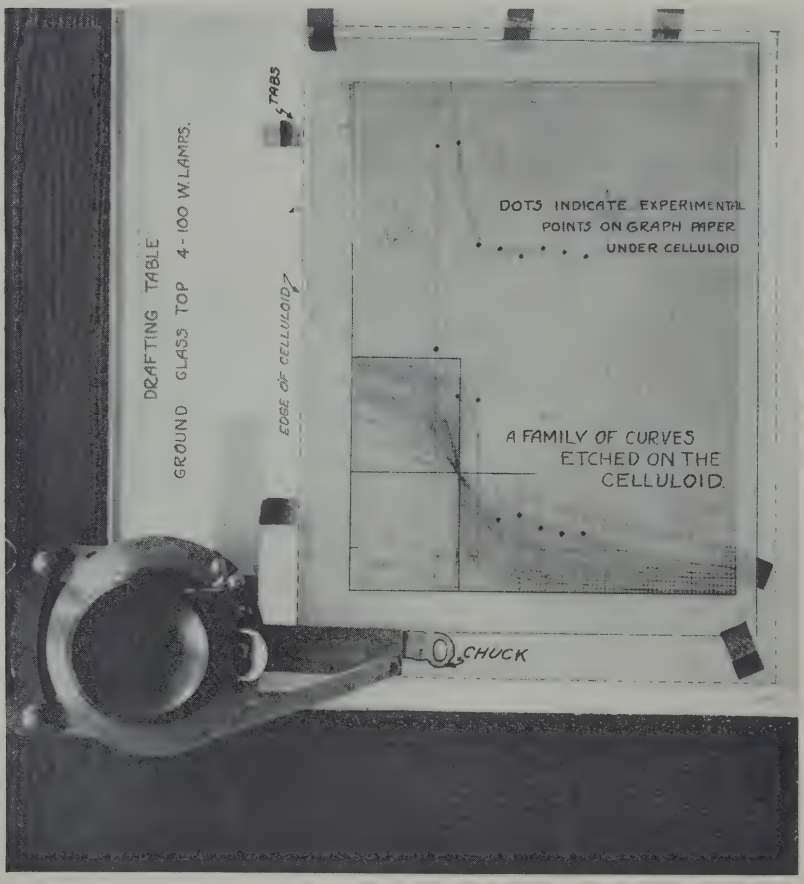
DRAFTING TABLE
GROUND GLASS TOP 4-100 W LAMPS.
TABS
EDGE OF CELLULOID
DOTS INDICATE EXPERIMENTAL POINTS ON GRAPH PAPER UNDER CELLULOID
A FAMILY OF CURVES ETCHED ON THE CELLULOID.
CHUCK

FIGURE 1

A Detail of the Curve Fitting Instrument

The Protractor of the Universal Drafting Machine is Visible at the Lower Left of the Cut. Explanation of Use in the Text.

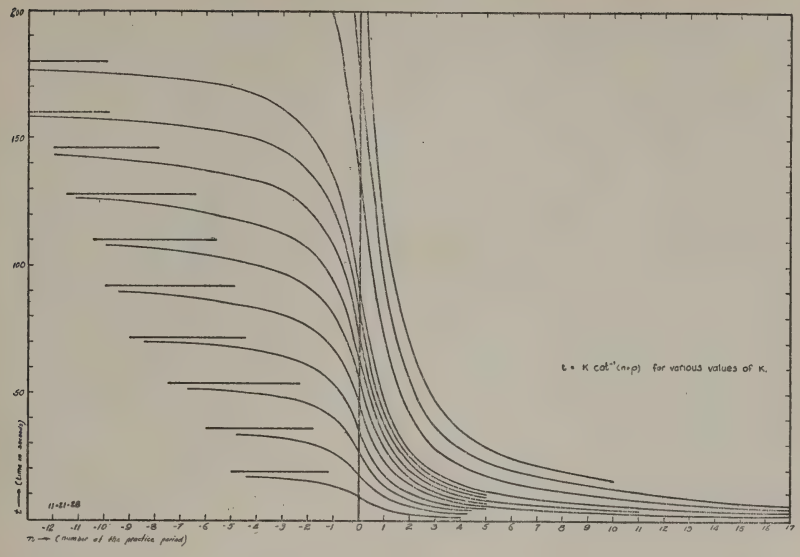

FIGURE 2
THE *arc cot* FUNCTION FOR VARIOUS VALUES OF k

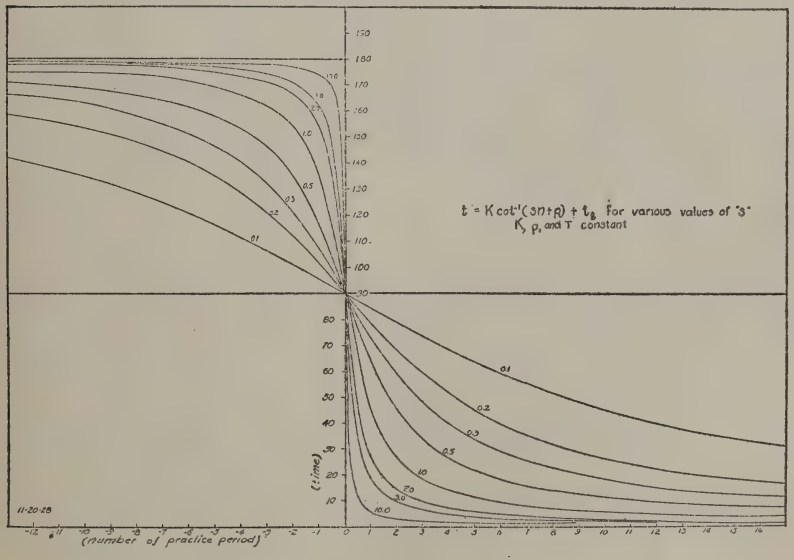

FIGURE 3
THE MODIFIED *arc cot* FUNCTION FOR VARIOUS VALUES OF s

and the celluloid pattern of the family of curves is put in its place. The same kind of chuck is used on the celluloid as is used on the straightedge and it is secured to the celluloid by means of collodion, clamped until hardened.

The Universal Drafting Machine assures that, however moved, the coordinates of the pattern will always remain exactly parallel to the corresponding coordinates of the graph paper.

To fit a curve: Choose a celluloid pattern containing the approximately correct value of k, move it up and down, right and left, until one of the curves fits the data. Read from the graph paper enough t-values to plot a curve. The value is determined by the intersection of the chosen curve with the ordinates of the graph paper. (As a matter of technique it is better to reverse the celluloid in etching the curves, so that the thickness of the sheet will not be interposed between the graph paper and the pattern.) The value of p is read directly from the graph paper by noting the value of n upon which $n = 0$ of the pattern falls. If $n = 0$ of the pattern falls to the right of $n = 0$ of the graph paper, p is negative; if to the left, p is positive. The value of t_1 is found by noting the t-value of the graph paper upon which $t = 0$ of the pattern falls. And, of course, k is read directly from the pattern.

The value of the instrument becomes even more apparent when a modified Meyer learning function is used. Here the equation is:

$$t = k \; arc \; cot \; (sn + p_1) + t_1$$

The nature of these curves for various values of s and k is shown in Figure 3. Four parameters must be evaluated now, so that the time saved becomes even more apparent. The instrument has been in use several months and has performed satisfactorily not only for fitting learning curves by means of arc cotangent functions, but also by hyperbolic and exponential functions of various types.

Reference

1. Meyer, M. F., & Eppwright, F. O. The equation of the learning function. *Amer. J. Psychol.*, 1923, **34**, 203-222.

Ohio State University
Columbus, Ohio

THE COMPARATIVE LEGIBILITY OF BLACK AND COLORED NUMBERS ON COLORED AND BLACK BACKGROUNDS

Ruth Miyake, Jack W. Dunlap, and Edward E. Cureton

The use of colored paper for notes, filing, indexing, etc., raises questions regarding legibility and reading ease of material written or printed on such

backgrounds. The reverse of the problem has been present for years in our public schools where material is written with colored chalks on a dark background, usually black.

The purpose of this paper is to present the results of a preliminary survey of this problem to see if significant differences appear and to suggest problems for more careful investigations.

Numerals from 1 to 9 were typed with a new black ribbon on three slips each of red, green, yellow, and white paper, the order of the numerals being random, and different for each of the 12 slips. The paper was all of the same texture and approximately of the same saturation and intensity for each of the colors. A second series of 12 strips of black paper was prepared by typing each strip without the ribbon, and then tracing the impressions of each set of three with red, green, yellow, or white ink, the numerals being arranged in random order such that no strip of the second series bore the same order as any other or as any of the first series.

The numerals were presented singly by means of a spring tachistoscope.

TABLE 1

SCORES OF SUBJECTS BY SERIES AND COLOR

	Series I				Series II			
Subject	Red	Green	Yellow	White	Red	Green	Yellow	White
A	27	27	27	27	23	20	27	27
B	27	27	27	27	0	0	23	27
C	27	27	27	27	3	10	24	27
D	25	27	27	27	6	8	21	27
E	26	27	27	27	11	1	25	27
F	0	19	25	27	0	1	1	27
G	25	27	27	27	1	0	20	27
H	26	26	27	27	4	3	23	27
I	24	27	27	27	1	7	26	27
J	23	27	27	27	1	3	23	27
K	20	26	26	27	3	1	21	27
L	25	27	27	27	0	1	27	27
M	18	27	27	27	0	0	21	26
N	25	27	27	27	0	0	20	27
O	27	26	27	27	0	0	16	27
Mean	23.00	26.27	26.80	27.00	3.53	3.67	21.20	26.94

The problem was now, essentially, to determine if differences between the various pairs of means for each series were significant. The ordinary probable error technique, resting on the assumption that means are normally distributed, is not applicable to samples as small as 15. Fisher (1) describes more accurate methods for treating small samples.

The speed of the instrument was so regulated, during a set of trials on a few preliminary subjects, that every subject was able to recognize correctly at least one numeral out of every nine in the second series. The actual exposure-time was not determined.

All subjects were seated at the same table, with the light coming over the left shoulder. All sittings were conducted on bright, sunny days between 9:00 and 11:00 A.M. to equalize the intensity of illumination as far as possible. Nine trial exposures were given to each subject, using a special white strip. At the experimental sitting for each series, the subject recorded his observations on a sheet of paper. If he was not sure what numeral he had seen, he might guess. If he saw nothing, he left the appropriate space blank. The score for each color was the number of numerals correctly recorded for the three strips. The order of presentation for each series was white, red, green, yellow, white, red, green, yellow, white, red, green, yellow. Each subject was given the second series two days after the first. Table 1 shows the scores of all subjects for each series and color, together with the means.

Using this technique, the differences between the corresponding scores of each color pair in each series were determined. The mean and standard deviation for each series of differences were computed. It should be noted that the mean of each series of differences equals the difference between the means of the two corresponding score series, and our problem was then to test whether the means of the differences were significantly different from zero. To do this, the measure t was computed for each series of differences, and the probability that a mean difference as great or greater than that observed would arise by chance was determined from Fisher's table of t. Probabilities less than .05 are usually interpreted as indicating significant differences. The formulas used were as follows:

For any series of differences, $\overline{d}$, let
$\overline{d}$ be the observed mean difference,
s the observed standard deviation,
t the value used in entering the table
p the probability that $\overline{d}$ arose by chance on the hypothesis that the true difference is zero,
n the number of subjects (here 15).

$$\text{Where } \overline{d} = \frac{\Sigma d}{n},$$

$$s = \left(\frac{\Sigma d^2 - n\,\overline{d}^2}{n-1}\right)^{\frac{1}{2}}$$

$$t = \frac{\bar{d}\sqrt{n}}{s}$$

Table 2 summarizes the results.

TABLE 2
SIGNIFICANCE OF MEAN DIFFERENCES

	$\bar{d}$	t	p
	Series I		
M_G—M_R	3.27	2.496	.028
M_Y—M_R	3.80	2.310	.040
M_W—M_R	4.00	2.254	.046
M_Y—M_G	.53	1.299	.217*
M_W—M_G	.73	1.377	.200*
M_W—M_Y	.20	1.355	.190*
	Series II		
M_G—M_R	.16	.160	.870*
M_Y—M_R	17.69	9.730	.01
M_W—M_R	23.40	14.745	.01
M_Y—M_G	17.53	10.348	.01
M_W—M_G	23.26	16.232	.01
M_W—M_Y	5.74	3.517	.01

*Difference not significant.

Table 2 shows that in Series I there are significant differences between red and each of the other colors, and in Series II the only difference which is not significant is that between red and green. All the others in Series II, except the last, approach certainty in significance. Fisher's table does not give probabilities below .01 (which corresponds to a t of 2.977); hence the actual probabilities, which obviously are extremely small, are not shown.

Since 8 of the 12 comparisons indicate significant differences, the writers believe that more careful investigation would throw much light on various problems dealing with legibility. The relatively poor legibility of black letters on red or for red letters on black is clearly indicated.

REFERENCE

1. FISHER, R. A. Statistical methods for research workers. London: Oliver & Boyd, 1925.

Territorial Normal and Training School
Honolulu, Hawaii

BOOKS

EDITED BY

KARL BÜHLER
W. J. CROZIER
JAMES DREVER
A. MICHOTTE
JOHN PAUL NAFE
A. L. SCHNIERMANN
RAYMOND H. WHEELER

BORING, E. G. *A History of Experimental Psychology.* New York: Century, 1929. Pp. xvi+699.

MURCHISON, C. [ED.] *The Foundations of Experimental Psychology* Worcester, Mass.: Clark Univ. Press, 1929. Pp. x+907.

The almost simultaneous appearance of these two volumes affords an opportunity to assess both the achievements of psychology during the 70 years in which it has been an experimental science, and also the problems uppermost in its present program of research.

Professor Boring's book is unique because it is the first among the histories of psychology to confine itself to the pursuits of psychological laboratories as distinct from those of the casually observant and sometimes genial philosopher. It is significant because it enables us to witness in perspective the borrowing of subjects for experimentation from the physiologists and philosophers which led to the segregation of a group of problems which the experimental psychologist might call his own. Though his theme is experimental psychology, the author has not neglected early observations and ideas concerning the mind and its functions which gave rise to such doctrines as associationism, phrenology, mesmerism, and the "personal equation." The foundation of experimental psychology, however, is attributed to Fechner, Wundt, Brentano, Stumpf, and G. E. Müller. It is from these worthies that we derive the classical rubrics of the science: psychophysical measurement, sensation, perception, attention, feeling, and the mental functions.

"Modern" as distinct from "classical" experimental psychology, if such a distinction is possible within so brief a temporal span, pushed forward, under the leadership of Ebbinghaus, into the investigaton of the "higher" mental processes of memory and association. G. E. Müller, under whose leadership this chapter in experimental psychology was rounded out, appears among the founders as a psychophysicist. It was Külpe and his school at

Würzburg who carried the investigation into the field of thought and thinking, thus adding a fourth category of impalpable elements to the already existing categories of sensation, feeling, and image. But this extension of the kinds of consciousness which an experimental discription might employ led to such vigorous controversy that, when the smoke of battle had cleared, the newer movements of functional psychology, behaviorism, and Gestalt had shifted the focus of experimentation, while at the same time the mental tests had arrived to divert the interests of many who might otherwise have been trained, as their predecessors had been, in the exacting methods of introspection. It is with an admirable sense of impartiality that Boring has traced the course of psychological experiment during these last decades. As a result he gives a clearly written and vivacious account of our present status which no serious student of psychology can afford to neglect.

It is with the present status of our science that the book edited by Professor Murchison deals. In his brief preface the editor makes the following statement of his aim:

"It has not been easy to determine the chapters that should appear in this book. The list as it appears has been decided upon after consultation with many who are interested in this type of approach to the problems of psychology. It is not likely that any fruitful problem has been omitted. Those problems that have been omitted are thereby classified as promising little return from experimental effort. I know no better way of clearing the débris from the intellectual decks of psychology. It has too long been the custom to discuss over and over every problem that has ever been discussed before, as if some value had accrued from mere discussion. The only criterion of inclusion that has been considered during the preparation of this volume is whether a given problem promises a reward to experimental investigation."

Here, then, we are to find, both in summary and prognosis, those problems which are of vital significance, and, with Boring's historical perspective in view, we shall be able to see at once what débris has been excluded as "promising little return from experimental effort."

The list of papers follows: 1) "The Mechanism and Law of Heredity"—T. H. Morgan, geneticist. 2) "The Study of Living Organisms"—W. J. Crozier, physiologist. 3) "The Mechanism of Reaction"—Alexander Forbes, physiologist. 4) "Vision: I. Visual Phenomena and their Stimulus"—L. T. Troland, psychologist. 5) "Vision: II. The Nature of the Photoreceptor Process"—Selig Hecht, bio-physicist. 6) "Hearing: I"—H. Banister, psychologist. 7) "Hearing: II"—H. Hartridge, physician, and H. Banister. 8) "The Chemical Senses"—G. H. Parker, zoölogist, and W. J. Crozier. 9) "The Sense of Feeling"—J. P. Nafe, psychologist. 10) "Space and the Non-Auditory Labyrinth"—J. Q. Holsopple, psychologist. 11) "Hunger and Thirst"—W. B. Cannon, physiologist. 12) "Emotion: I. The Neurohumoral Basis of Emotion"—P. Bard, biologist. 13) "Emotion: II. The Expressions of Emotion"—C. Landis, psychologist. 14) "Learning: I. Nervous Mechanisms in Learning"—K. S. Lashley, psycho-biologist. 15) "Learning: II. Experimental Studies of Learning"—W. S. Hunter, psychologist. 16) "The

Individual in Infancy"—Arnold Gesell, psychologist. 17) "The Individual in School: I. General Ability"—R. Pintner, educational psychologist. 18) "The Individual in School: II. Special Abilities and their Measurement"—F. N. Freeman, educational psychologist. 19) "The Adult in the Community"—M. A. May, educational psychologist. 20) "The Conflict and Survival of Cultures"—C. Wissler, anthropologist. 21) "The Abnormal Individual"—S. I. Franz, psychologist. 22) "General Statistical Principles"—T. L. Kelly and E. Shen, psychological statisticians. 23) "The Statistical Treatment of Certain Typical Problems"—T. L. Kelley and E. Shen, psychological statisticians.

Of these authors seven are not listed in *The Psychological Register,* which Professor Murchison edits, and at least two others belong primarily in other fields than psychology.

Of greater interest is the list of topics from which "it is not likely that any fruitful problem has been omitted." Of those topics, which, according to Boring, were the chief centers of interest to the founders of our science, only two have survived in this compilation, namely, *sensation* and *emotion.* Psychophysics, which was to make a science of psychology, is referred to twice, once as an example of statistical method, and again in Troland's discussion of brilliance and chroma. Perception, which is still thought by some to be a lively topic, appears only incidentally in this volume. Troland tells us that "but little is gained by following the traditional terminology of sensation and perception." Banister uses the term in speaking of the perception of tone and the perception of auditory space. Holsopple calls attention to the lack of perceptual data in the phenomena of vestibular orientation, and Gesell has a brief section on the perceptual behavior of infants, in which the earliest responses to light and sound are described. Otherwise, perception is relegated to the fruitless problems. Attention is in still worse straits, for this term does not appear at all in the subject-index. Memory, imagery, imagination, and association are also discarded topics. Hunter, to be sure, has a brief section on "Reminiscence" in which some memory curves of children are given, and the inadequacy of our explanations of reminiscence is pointed out. But considering the immense amount of experimental work which the subject *memory* has inspired in the past, its present viability would seem to be highly questionable. As for imagery and imagination, they find no place in the index, while association appears but once as an explanation of expressive movements, such as a wry face. The higher mental processes of judgment, thought, and thinking have entirely disappeared. What is left may be summarized as topics in biology, sense-physiology, education, and statistics.

We have already noted that 9 of our 23 authors are not psychologists. It is also of interest to note the sources from which our authors have drawn the materials of their discussions. The names indexed at the end of the volume number some 1212 persons, of whom 257 are listed in *The Psychological Register* and 31 others are recognizable as psychologists,

mostly deceased. In other words, less than one-quarter of the references in this volume is to the work of psychologists.

At the end of each paper there appears a useful bibliography ranging from 15 to 220 titles. Only 9 of the 23 bibliographies refer to psychological publications in excess of other work. Troland cites 39 psychological studies in a total of 76 titles; Nafe, 33 in 61; Holsopple, 11 in 16; Landis, 38 in 49; Hunter, 120 in 123; Gesell, 31 in 32; Pintner, 82, all psychological; Freeman, 70, all psychological; and May, 31 in 41. From this analysis it would appear that the significant experimental work of the psychologist today is in the fields of sensation, emotion, problems of learning, educational measurements, and social psychology. Yet the psychologist's contribution to our knowledge of sensation and emotion is not so apparent if we take the bibliographies of Hecht, Banister, Hartridge, Parker, Crozier, Cannon, and Bard as our guides. Thus, the net result of our inquiry appears to be that the only fruitful problem which the present-day psychologist can call his own is that of learning and its measurements. When we consider further that this topic of investigation is largely a matter of statistical interpretations of empirical rather than of psychologically factual data, our conclusion seems to be that psychology as a science is now bankrupt. Having ceased to claim a distinctive field of research, it appears to have become an adjunct of teacher-training bent upon economical means of learning how to behave as social custom dictates.

If we turn now to Boring we shall find that he too has doubts concerning the effectiveness of psychology as a science. While brave in his belief "that the application of the experimental method to the problem of mind is the great outstanding event in the history of the study of mind," he confesses "to a certain disappointment . . . that experimental psychology has not accomplished more than it has in its seventy years of life." What are the reasons? He gives us two: first, that psychology "has never had a great man to itself," and, secondly, that it "has never succeeded in taking philosophy to itself nor in leaving it alone."

One wonders. Why should no great man have appeared among the psychologists? Can it perhaps be that the subject itself is unsuited to genius? And can we be assured that "psychology ought to fare better when it can completely surrender its philosophical heritage, in fact as well as in voiced principle, and proceed, unimpeded by a divided soul, about its business?" One wonders.

In Germany, the home of experimental psychology, philosophers are now succeeding psychologists in chairs which have formerly carried with them the direction of psychological laboratories. In England, experimental psychologists are still few in number and ill-equipped for their work. In America, where no college is too small to boast a psychological laboratory, the apparatus is rusting while the experimentalist turns to tests and statistics.

If the achievements of the psychological laboratories are disappointing,

and at best only such as might have come anyway from the workshops of the physicist, the biologist, and the educationist, ought we not to counsel the frank recognition of "philosophy as dependent upon science" which motivated Wundt in his experimentation, rather than turn our backs still more definitely upon a philosophical criticism of our methods and results? "Ever since their foundation," writes Boring, "the journals of psychology in all countries have been weighted down with 'theoretical' papers that are really expositions of psychologists, untrained to philosophy, but writing on philosophical matters." True, but why not give our psychologists a philosophical training such as the Germans have always had? Then, at least, they will be critical of their experimental findings, and perhaps be saved much useless effort in the planning and carrying out of elaborate experiments the results of which have no meaning. If experimental psychology is really bankrupt, as Professor Murchison's volume seems to indicate, why should we not be frank enough to recognize the fact and pass voluntarily into the hands of our receivers, the biologists and the statisticians? Those who are left over might conceivably render service by revitalizing a field of research in which at the present time there recurs the belief of Wundt that philosophy is dependent upon science.

R. M. OGDEN

Cornell University
Ithaca, New York

BRIDGES, J. W. Psychology, normal and abnormal. New York: Appleton, 1930. Pp. xxii+552.

CONKLIN, E. S. The psychology of religious adjustment. New York: Macmillan, 1929. Pp. xiv+340. $2.00.

FISHBACK, E. H. Character building for junior–high-school grades. Boston, Mass.: Heath, 1930. Pp. vii+232.

DELLA MIRANDOLA, GIANFRANCESCO PICO. On the imagination. (Latin text, with an introduction, an English translation, and notes by Harry Caplan.) *Cornell Stud. English,* 1930, **16**. Pp. 102. New Haven, Conn.: Yale Univ. Press. $1.00.

MOSS, F. A. Applications of psychology. Boston: Houghton Mifflin, 1929. Pp. xx+477.

PIÉRON, H. Le développement mental et l'intelligence. Paris: Alcan, 1929. Pp. xii+95.

REMMERS, H. H., & BRANDENBURG, G. C. A syllabus work-out for a two-level plan of instruction in elementary psychology. New York: Macmillan, 1930. Pp. vii+221. $1.80.

SELIGMAN, E. R. A., & JOHNSON, A. [EDS.] Encyclopaedia of the social sciences. Volume I: Aar–All. New York: Macmillan, 1930. Pp. xxvii+646. $7.50.

WATSON, G., & SPENCE, R. B. Educational problems for psychological study. New York: Macmillan, 1930. Pp. xii+353. $1.80.